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VOL. XXXVIII NO. 4 PSYCHOLOGICAL REVIEW PUBLICATIONS WHOLE NO. 176 1928

Psychological Monographs

EDITED BY

RAYMOND DODGE, Yale University
HOWARD C. WARREN, Princeton University (*Review*)
MADISON BENTLEY, Cornell University (*J. of Exp. Psych.*)
S. W. FERNBERGER, University of Pennsylvania (*Bulletin*)
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UNIVERSITY OF IOWA STUDIES IN PSYCHOLOGY

No. XI

EDITED BY

CHRISTIAN A. RUCKMICK

Published for The American Psychological Association by
PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
and ALBANY, N. Y.

Psychological Review Publications
of the
American Psychological Association

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NOTE

Having served as editor of these Studies for twenty-five years, I feel that it is time for someone else to take a turn at it, and in the natural division of labor within the department it is fortunate that Professor Ruckmick, who devotes all of his time to experimental psychology from the laboratory point of view, is available and willing to undertake this work. I turn over the editorship to him with best wishes.

Carl E. Seashore

PREFACE

This volume presents for the most part a series of experimental investigations carried out by the respective authors as theses in partial fulfillment of the requirements for advanced degrees. The material has been considerably condensed in every case from the original thesis form, and since the editor shares with the authors the responsibility of slight revision and much abbreviation which resulted, the reader will understand that all of the unsupported statements made are based on extensive protocols on file in the laboratory. Several of the articles are in the form of preliminary studies which have not been so severely condensed.

The fields investigated comprise for the most part the auditory phenomena connected with sound localization, with vocal expression, and with many problems in the field of musical talent. Several of these studies are also concerned with emotional responses brought under the control of laboratory experiments.

While each paper represents work done in this laboratory, almost without exception there has been cordial coöperation between the department of psychology and a number of other departments in the University by way of devising suitable apparatus or contributing material needed for experiments.

The editor wishes, therefore, to make grateful acknowledgment to his colleagues in other departments who have assisted his department in bringing to bear the most recent technique and theoretical interpretation available in the affiliated sciences. He is indebted to the graduate students who have materially helped him in the preparation of their respective studies for publication. He also wishes to express his appreciation of the cordial coöperation of Dean Carl E. Seashore, who has directed several of the investigations reported and has coöperated with the editor in the direction of the remaining studies as indicated in each case. He has also been of invaluable assistance in giving the editor the benefit of his experience gained over a long period of years as the editor of the first ten numbers in this series of studies.

THE EDITOR

CONTENTS

A STUDY OF FEAR BY MEANS OF THE PSYCHO-GALVANIC TECHNIQUE [1]

by

Nancy Bayley

Introduction; experimental studies; psychogalvanic studies of pain and fear; apparatus; observers; preliminary adjustment and instructions; affective situations; treatment of the psychogalvanic results; the relation of the breathing changes to the psychogalvanic reflex; introspections of fear; comparison between introspective and psychogalvanic results; conclusions; bibliography.

Introduction. The emotion of fear, like all emotions, is elusive under experimentation. For this reason, and also because it is only comparatively recently that the emotions have been studied experimentally, very little is known of the real nature of fear. Fear is described variously according to the different theories of the emotions which have been advanced. *Hall* (**44**), for example, states: "Fear is the anticipation of pain." He goes on to say that without conservation of past experience (both racial and individual) there could be no fear. It causes physiological changes which better fit the individual for fight or flight. In connection with fear he discusses shock: "Here there is no anticipation of pain—but the pain is sprung without warning." "The utterly new and sudden is always dangerous because we lack the apparatus to react fitly to it and so lapse to elementary responses." This instinctive basis is stressed more by *Mc-Dougall* (**60**). Linking the emotion of fear with the instinct of flight, he says: "Fear, whether its impulse be to flight or to concealment, is characterized by the fact that its excitement, more than that of any other instinct, tends to bring to an end at once all other mental activity, riveting the attention upon its object to the exclusion of all others."

Sully (**98**) defines fear as "the particular emotive reaction which takes place through a sufficiently vivid and persistent repre-

[1] This study was completed under the direction of Dr. Christian A. Ruckmick.

sentation of possible pain or evil." In agreement with this definition, *Pillsbury* (74) stresses the similarity between emotions, and shows how readily they may be interchanged. "Anger is distinct from fear only in the consciousness of power or weakness toward the intruding man or object, and this changes from moment to moment as the situation is faced."

Somewhat in agreement with Hall's distinction of shock from anticipatory fear, *Ribot* (80) includes two stages of fear—a " primary instinctive unreasoning fear preceding all individual experience, and a secondary conscious reasoned fear posterior to experience. *Dunlap* (22), in distinguishing between the quickly aroused emotive responses of the striped muscles and the slowly aroused emotive responses of the viscera, shows how they may both be aroused in fear. " In some cases," he says, " the quick component occurs, with the slow component so minimal that we call the complex not fear, but 'being startled.' "

Among the behaviorists, we find *Watson* (124) limiting his description to unlearned fear responses of children. He gives as the causes of fear, loss of support, and loud sounds. The response is " check-of-breathing, 'jump,' or start of whole body, crying, often defaecation and urination (and many others not worked out experimentally. Probably the largest group of part reactions are visceral)." He says of emotion in general: "The shock of an emotional stimulus throws the entire organism for the moment at least into a chaotic state."

From the foregoing statements it is seen that psychologists include descriptions of both mental processes and physical responses. Some stress one and some the other. It is generally conceded, however, that physical response of some kind occurs with all emotion. In this we see evidence of the profound influence of the James-Lange theory of the emotions.

These various definitions raise three important questions: (1) Is fear simple or complex? (2) Are there different forms of fear? (3) To what extent are these forms due to modification? What fears are inherited as such? We will concern ourselves primarily with an answer to the second question.

The definitions seem to indicate a general inclusive term "fear." In the narrower sense it designates the anticipation of an impending evil whose advent is certain. Dread, however, seems to be descriptive of the contemplation of the possible occurrence of an evil which would be fearful if certain. Terror, then, is extreme fear. Horror adds to fear the implication of aversion, as when watching extreme suffering or torture of another. Startle, fright, and alarm are different degrees of a fear caused by a sudden unexpected stimulus. These are shocks causing a reflex recoil, and are ordinarily short-lived. Watson's description of fear in infants would be entirely of this "startle."

The purpose of the present study is to investigate some of the different types of fear, to determine if possible some basis for these differentiations, and to throw some light on the nature of the emotion or emotions of fear.

Experimental studies. Only a comparatively small number of experimental studies of fear as such have appeared. Many of the older theoretical studies of fear are based largely on the author's own introspections, but these were not made under controlled conditions, and are formed on only one individual's report. An introspective study made by *Conklin* and *Dimmick* (16) gives results of some value in regard to the mental processes in fear. A number of fearful stimuli were presented to three girl students and reports secured from them. They conclude that the "presence of fear does not depend on any particular group of processes such as organic sensations, but rather on some perceptual meaning of the object for the observer." It was found that fear could be produced by giving an imaginal setting before the stimulus was presented.[2]

Physiologists have studied internal bodily changes. *Cannon* (13) devotes many of his experiments to the fear reactions of animals, mostly of cats and dogs. He found identical changes for pain, fear, and rage. These were secretion of adrenin, increased sugar

[2] We have, incidentally, secured galvanic deflections by merely requesting *O* to feel anger, fear, or some other emotion through verbal suggestion of the name of the emotion. One highly trained musician responded very definitely to this instruction.

content in bood and urine, more rapid coagulation of the blood, and redistribution of blood to the skeletal muscles. *Crile* (18) shows that the great excitement of intense fear causes evident exhaustion and fatigue of the nerve cells of the cortex.

In the expressive method of measuring affection there are several procedures. One used frequently in the detection of fear is that of measuring changes in blood pressure. This has been used successfully in the detection of falsehood by *Landis* and *Gullette* (55), *Larson* (56), and *Marston* (59). The emotion aroused here is probably a fear of being detected, and is anticipatory in nature. Changes in breathing are subject to causes other than emotional to such an extent that pneumographic records are not very accurate. However, work on the inspiration expiration ratio by *Burtt* 12), *Landis* (55), and *Larson* (56) gave some evidence of its possible value in detecting crime. *Moore* (61) has measured emotion in terms of *O*'s ability to do mental computation while experiencing an emotion. The time taken in solving the problems was recorded, and used as an index of the amount of emotional disturbance. He found that fear caused by far the most powerful disturbances in the thought processes involved. This may have been due in part to the fact that his fear stimuli were more effective than the others, and it may indicate that fear is more distracting to mental processes than are other emotions.

Psychogalvanic studies of pain and fear. An objective measurement which has promised to be fruitful is that of the psychogalvanic reflex. The psychogalvanic reflex is a change of electrical conduction through the body which can be measured by means of a galvanometer. By most investigators it is assumed to be due to changes in resistance at the skin. It occurs under emotional conditions and when an emotion is reported by *O* (75, 109, 129).

In the various studies of the psychogalvanic reflex pain and fear producing stimuli have been frequently used for eliciting emotions. These involve sudden loud noises, pin pricks, slap on face, threat of burn, and the like. The deflections resulting from

these stimuli were on the whole of greater amplitude than those occasioned by pleasant stimuli. A few psychogalvanic studies have been made which bear directly on the emotion of fear.

Waller (**110**) took a continuous reading during a German air raid in London. The subject was a middle aged woman. She sat in an arm chair and read a quiet book. The warning signal caused a slight response, and a very slight response was made to the sound of the defense guns, but a large response was made to the hum of the supposed German planes and explosions. There was also a response to the siren at 1:30 P.M. signalling the end of the raid. This was due to its unexpectedness and novelty.

Blatz (**8**) studied fear caused by sudden release of support. His *O*s were fastened in a chair which could be made to fall backward suddenly. After three adaptation periods the chair was released without warning. It was released again during subsequent readings. The *O*s showed augmentation of heart beat lasting over 6 min., retardation of breathing, and deeper breathing, and a striking increase of e.m.f. with a latent period of from .5 to 3 seconds and lasting from 1 to 6 minutes. Repetition reduced all effects in degree and duration. The *O*s reported fear on the first fall but not on the expected ones. Blatz concludes that a complex organic response and a gross muscular adaptive force is necessary in order that *O* label the experience "fear."

The first of these two experiments is for the most part of an anticipatory fear, and the second is a startle. The question remains, are they the same emotion?

In order to study the nature of different types of fear, we have combined the measurement of the psychogalvanic reflex and the respiratory response with introspective reports of the *O*s.

Apparatus. For this experiment we used a Leeds and Northrop (Cat. No. 2285-S) type H mirror galvanometer with a sensitivity of 381 megs. and resistance of 12.5 ohms, and damped with a 30 ohm resistance across the two poles. The connections are shown diagramatically in Fig. 1, while the arrangement of materials and actual wiring are shown in Fig. 2. A variable

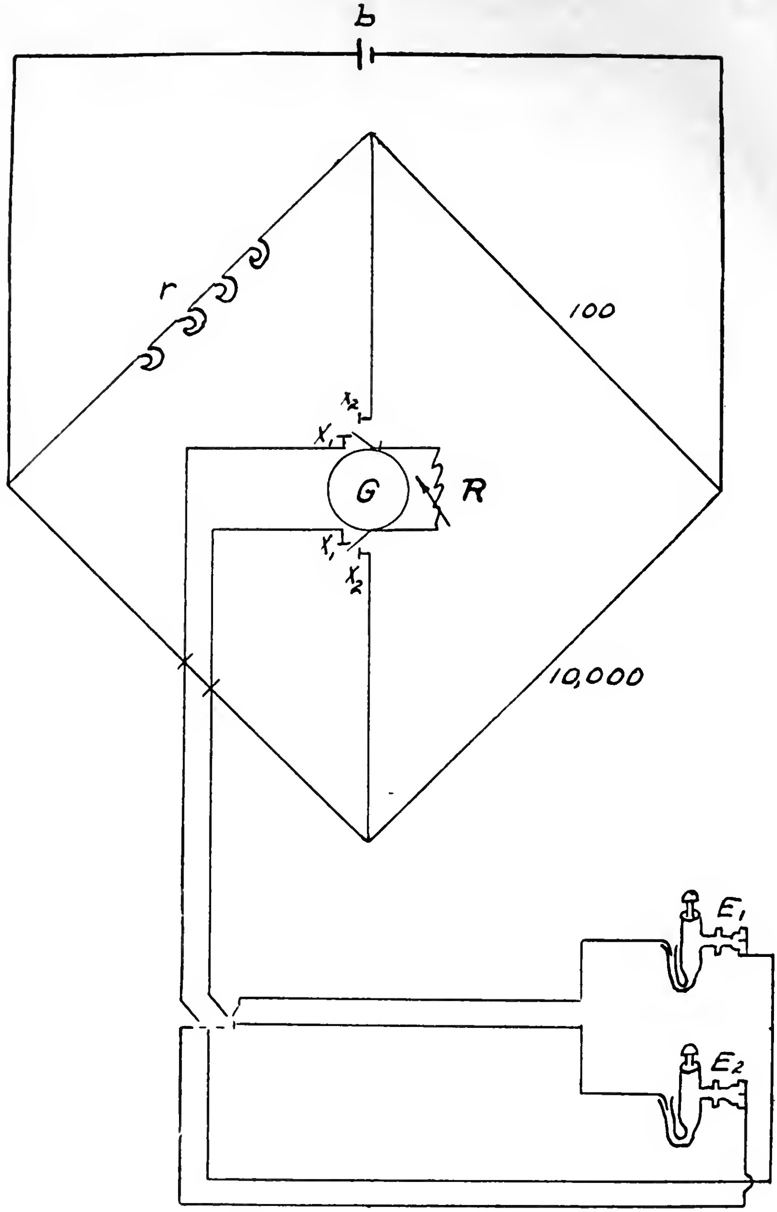

Fig. 1. Schematic drawing of circuit through bridge

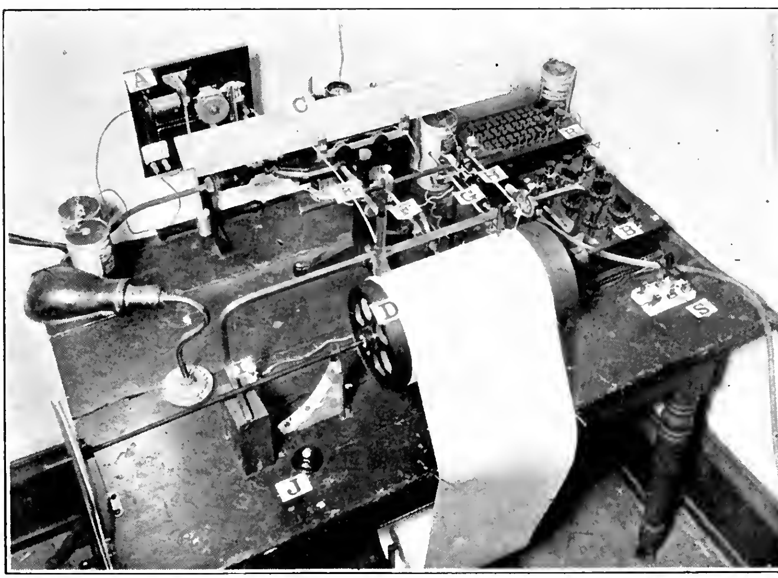

Plate I. Recording apparatus

Plate II. Apparatus for recording psychogalvanic reflex and breathing curve

resistance box (R) was connected across the galvanometer for the purpose of regulating its sensitivity. The current of a single dry cell (b) was applied to the galvanometer through a Wheatstone bridge (B). The third arm of the bridge was a variable rheostat (r), while *O* formed the unknown resistance of the fourth arm (o). This circuit is completed by closing the switches at x_2. A circuit through the galvanometer and *O* alone can be made by closing the switches at x_1.

The galvanometer was set up in a closet off from a small room in which the *O* sat (Plate II). The galvanometer was hung on the wall a meter above the recording material which was fastened to a table (Plate I). A light (L_1), directed on the galvanometer mirror, caused the excursions of the mirror to be

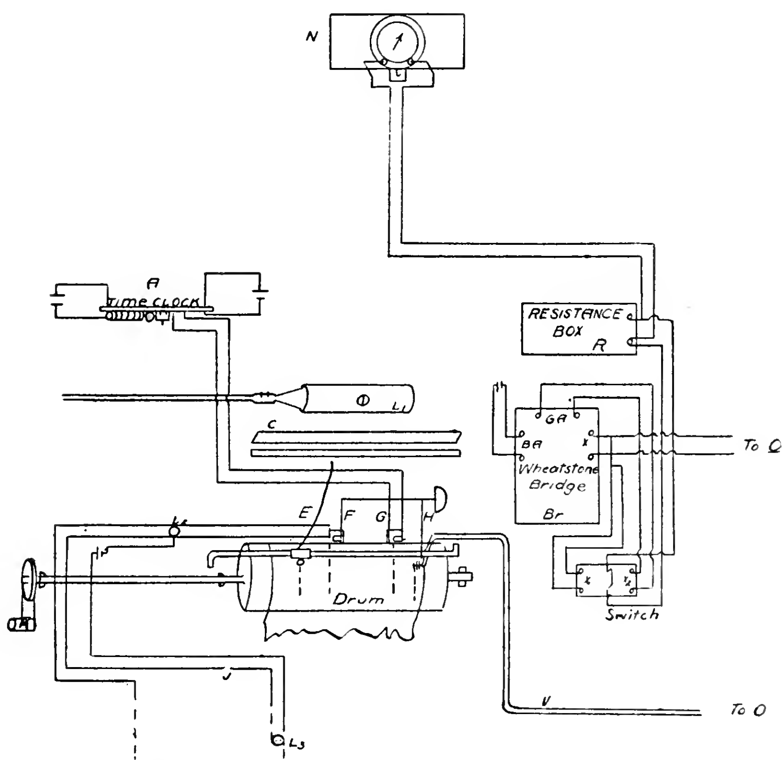

Fig. 2. Diagram of apparatus showing wiring

reflected on a transparent scale (C, Plate I and Fig. 2) by means of a lens and an angle mirror. The excursions of the mirror were recorded on a kymograph-drum (D) driven by an electric motor (M) which was geared down to a very slow rate of rotation. The deflections were followed by a fountain-pen in a holder which was made to slide on a rod parallel to the scale, and from which a wire (E) extended toward the scale as a guide. The pen recorded on white paper which wound onto the drum continuously from a roll. An electric clock (A) actuated a signal magnet (G) every two seconds, causing a jog in the ink-line made by an attached pen. A third pen traced a line which served as a position of reference and also as a signal marker. This pen was similarly attached to a signal magnet (F), and *E* operated it by a button (T) in the outer room at the time stimuli were presented.

During most of the experiments a breathing record was made at the same time as the galvanic record, by a writing point on a tambour at (H) which was connected to a Verdin thoracic pneumograph attached to *O*. There were, then, four lines being recorded simultaneously, a signal line, a time line, the breathing curve, and the galvanic deflection (Plate III).

Electric signal lights between the two rooms made it possible for *E* and *R* (Reader) to communicate without disturbing *O*. L_2 was in the same circuit as the signal magnet (F) and was actuated in the outer room by the switch or button (T). L_3 was the light in the outer room which *R* might turn on by pressing the button at J.

O completed the circuit through the galvanometer by inserting his hand between two "calomel" electrodes. These were used because liquid insures a more constant contact, and the mercury and mercurous-chloride combination excludes polarization changes inherent in the electrodes. The electrodes are shown in Plate II and in diagram form in Fig. 3.[3] The glass container was 14 cm. high with a rubber one-hole stopper through which was inserted

[3] Professor J. N. Pearce, of the department of physical chemistry, very kindly designed the electrodes.

a separatory funnel (K) for use in adding liquid. The contact tube (T) projecting at the side was 8.5 cm. long with an opening at (F) 6 mm. in diameter. To the bottom of the container a connecting tube (E) was attached from which a lead (H) went to the galvanometer. The connection through the glass was made

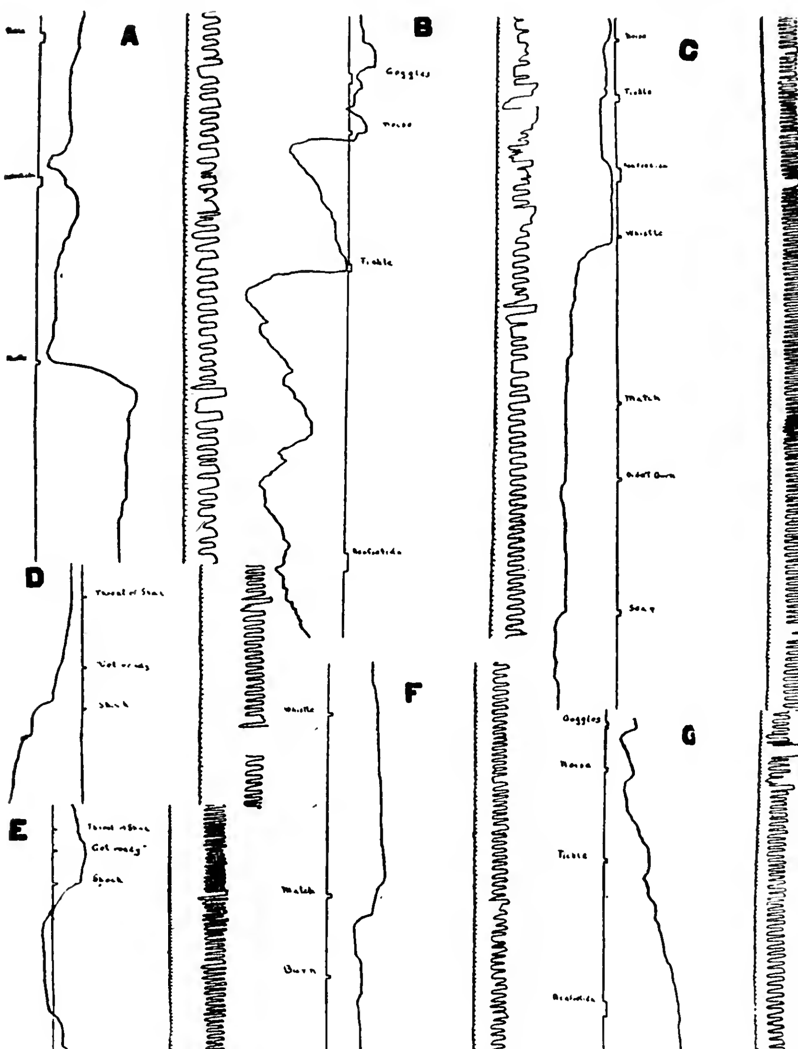

Plate III. Representative curves

by means of a platinum wire (D). In the container a layer of mercury (A) was covered by a layer of a paste of mercury and mercurous-chloride (B). The rest was filled by a 1 per cent sodium chloride solution (C). This latter formed a liquid contact at (F). The flow of the liquid was regulated by means of stop cocks at (S_1) and (S_2). The two electrodes were fastened to the palm and back of *O*'s hand by means of wooden holders placed back of the shoulder (G), and held together by strong rubber bands. The electrodes fitted into a wooden box which rested on the arm of a Morris chair in which *O* sat. The pressure of the contacts was relieved by brass discs 2½ cm. in diameter and ½ cm. thick with a hole in the center just large enough to slip over the glass tube at (F). This formed a larger pressure surface on the hand. It was also possible to connect these discs directly to the galvanometer if desired in place of the liquid electrodes to give an electrode of the type used by many investigators.

Observers. Of the twenty-five in this experiment ten were women and fifteen men. The women are designated as *By, Ca, Ge, Ha, Han, Ho, Pat, Pa Ru, Sp,* and *Wa.* The men were *Ba, Bl, Be, He, How, Lu, Me, Se, Th, Tr, To, Wag, Wi Wo,* and *Yo.* Most of them were students in the advanced laboratory class in psychology, and had therefore had several courses in psychology. They were graduate students with the exception of *Ha,* a professor in physical education, and *Pat* and *To,* who were juniors. Most of the *O*s had had some training in introspective observation, while *By* and *Pat* had considerably more training than the rest. They ranged in age between approximately eighteen and forty-five years.

Preliminary adjustment and instructions. *O* was seated with the left hand fastened into the electrodes on the chair arm, his hand having been first cleaned with a piece of cotton dipped in alcohol.[4] For the later experiments a Verdin thoracic pneumo-

[4] This was done to remove all impurities from the hand and to remove any excess moisture that might allow conduction over the surface of the skin.

graph was first adjusted on *O* for the breathing record. The stop cocks on the electrodes were opened, completing the circuit. *E* then gave *O* a typewritten paragraph of directions and a list of

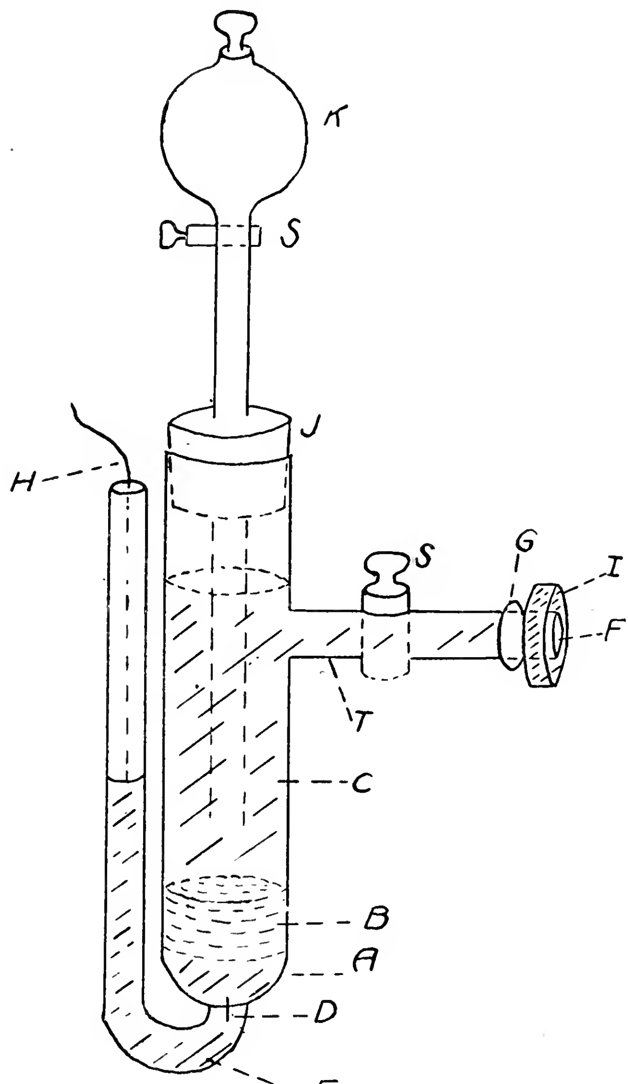

Fig. 3. Electrode

names of emotions with accompanying definitions to read. The directions read:

"Make yourself comfortable and sit quietly. Attend to whatever happens as you will be asked afterward to give a report of how each situation affected you. Take note of the kind of feeling produced, its intensity and duration. Name it if possible. Rate the intensity on a scale of 10 with 10 very intense, 5 moderately intense, and 1 indifferent. Note also associated ideas and imagery and any physical or physiological changes, as muscle strains, and relaxations and their location, as dryness of throat, moistness of palms, organic sensations, etc."

The definitions of emotions were given because it was found that the untrained *O*s were at a loss for words to describe their affections. The list was not nearly comprehensive, but was fairly full in the emotions which were related to fear, and which were of greatest importance in this study. Most of the definitions were taken from the Funk and Wagnell dictionary. They were verified and in some places slightly changed on the basis of definitions of a number of psychologists.

As soon as the galvanometer circuit was closed the shunt was adjusted and the Wheatstone bridge was balanced. With the writing points inked, and the time-clock going, *R* started the kymograph motor and took the record. *E* blindfolded *O* and gave the stimuli, actuating the signal magnet with each stimulus.

The records of the deflections were made on the drum by a reader who moved the pointer (E) back and forth on the rod to coincide with the mirror deflections. Four graduate students in psychology were trained and made all the readings with the exception of a few made by *E*.

Affective situations. A number of situations were arranged with the stimuli in a definite order. This order was followed for most of the *O*s, but not rigidly. Some *O*s observed five or six times, while others observed only once, so that quantitatively there are more records for some stimuli than for others. In addition to this, the effectiveness of the stimuli varied with the individual *O*s. This made it advisable to vary the situations at times.

The first situation was designed to call out rather elementary reactions, the stimuli being very simple. They were in order, noise (dropping of iron weight), tickle on lips with soft brush,

asafœtida under nose, shrill whistle, and holding a lighted match till it burned the finger. As a final stimulus *O* was told that nothing more would happen, but to sit quietly and rest for a minute. This is designated in the tables as "all over." The later situations involved more complex ideas and associations. Gruesome stories were read to work up a mental attitude in which emotions would be easily aroused.

The second sitting began with a "gong" (an A^1 tuning fork on a resonator). *E* then read aloud a gruesome story, "Solange," by A. Dumas. After this cool air was blown across *O*'s face with a fan; and finally a .38 revolver loaded with an extra loud blank cartridge was fired about 4 ft. from *O*.

In the third situation *O* read to himself a newspaper clipping which described in detail a severe punishment of criminals (referred to in the tables as "Cat o' nine tails"). Then *E* said to *O:* "I want you to estimate in inches the distance around your right wrist and the distance around your head at the largest part, and then tell me the difference between them. For each inch mistake you make you will receive a strong electric shock. I have a tape measure here and will make the measurements when you have given me your answer." The shocks were applied in accordance with the threat. This was the only sitting in which *O* was not blindfolded.

In the fourth situation *E* read aloud "The Cask of Amontillado," by E. A. Poe. Then *O* was told to multiply mentally the following problems and tell *E* the answer. They were: 4×13, 15×12, 12×18, 13×17, 236×27, 192×14, 364×27, 196×124. *O* was encouraged on the first ones but on failure with the fifth he was given an electric shock and threatened with another shock for each wrong answer. *E* continued this only until *O* had received punishment for this and had corrected one problem.

Situation five started with an oral quiz. *E* said "Professor Ruckmick has asked me to rate you in laboratory work, so I am going to take this opportunity to ask you some questions about laboratory apparatus and find what you have learned this year."

E then asked a number of questions which were hard enough that many of them could not be answered by the *O*s. After the quiz a bottle of formaldehyde was held under *O*'s nose, and then his hand put into a jar of soft soap. *E* then told *O* she wanted a blood test, cleaned the end of *O*'s finger with alcohol, and pricked it. This is referred to as "blood" and "prick." *O* was then told that the quiz was a farce and would not be used in the grades. The final stimulus was a piece of chocolate candy.

A few other stimuli were used on one or two *O*s. Two Victrola records, Kreisler's *Old Refrain,* and the *Erl König* sung by Schumann-Heinck, and an article describing cattle drowning at sea, were presented as stimuli to several *O*s. Three *O*s, *Ba, Ho,* and *Pat,* were given the gun to shoot themselves when *E* signalled.

Ten of the *O*s who observed only once were stimulated with noise, tickle, pin, asafetida, whistle, match, hand in soft soap, and gun. This series of readings was made after the others. In all the experiments except in situation three, *O* was blindfolded throughout the experiment. At the end of each series *O* gave his introspective report verbally to *E,* who wrote it down as it was given.

Deflections occurred a number of times at the beginning of the experiment before the series of situations was started. The uncertainty of what would happen next seemed to be an emotive situation here. These deflections are indicated in the tables under the stimulus column by the word "beginning."

Treatment of the psychogalvanic results. Plate III shows samples of the records as they were made during the observations. They lend themselves to a certain number of comparisons and assumptions as they stand, but can not be compared in regard to absolute magnitude of the deflections, for at each observation the bridge was balanced at a different resistance, as *O*s vary in body resistance.

In order to make comparisons of the reactions, the magnitude of each deflection of an *O* was multiplied by the resistance as measured by the Wheatstone bridge at the experimental hour. This made all the readings for one *O* comparable if the sensitivity

of the galvanometer was the same. The arithmetic mean of these deflections was then computed and the deflections translated into per cent. of the mean deflection,[5] giving a value which could be used in comparing the relative degree of reactions of all *O*s and with varying sensitivity in the galvanometer, to the various situations. For different galvanometer shunts the percentages were computed separately as for different *O*s. Table I gives the per cent. values in descending order of magnitude for each *O* with the accompanying stimulus. Since a large number of deflections was not secured from each *O* no exact statistical treatment would be valid, but the results give sufficiently large differences to indicate tendencies.

From Table I the scores in terms of per cent. of the mean for each stimulus were collected and the arithmetic mean of these

TABLE I. *Galvanic responses of Os listed in descending order of magnitude*

Score: Per cent of mean	Stimulus	Response
		Ba
162	problem	slightly bothered by attempt at correct answer
130	shock	jump and contraction, but no emotion
75	"Cat o' nine tails"	rather startled, then angry
25	problem end	
...	whistle	slightly stunned, a sort of fear
		Be
211	tickle	start, surprise, self disgust
150	whistle	very startled, surprise
115	match	apprehension
61	asafoetida	surprise, slightly unpleasant
41	noise	neutral, didn't jump
20	burn	pain
		Bl.
371	whistle	very startled
238	asafoetida	very repulsive
219	formaldehyde	disgusted
206	problem right	relief
132	arithmetic shock threat	apprehension, dread
91	exp. before asafoetida	
91	exp. before match	
88	gun	shock, startle
73	burn	

[5] Smith (**95**) in Appendix II explains and justifies the use of this method for comparisons of the psychogalvanic reflex for different *O*s.

Score: Per cent of mean	Stimulus	Response
73	tickle	slight laugh
66	" Solange "	sympathy, pleasant
39	noise	very slightly affective
36	match	apprehension of burn
36	" all over "	relief
30, 98	problem	anxiety, apprehension
19	shocks	pain
15	" Cat 'o nine tails "	anger
14	after " Solange "	
7	"Amontillado "	very slight horror

Ca

—321	noise	surprise, slight jump, not very bad
—192	whistle	startled more than noise, shrill, sharp
—73	beginning	
—64	asafoetida	rather repugnant, not pleasant
—28	goggles	
—9	match	didn't bother
—9	all over	expected more
...	burn	sharp hot feeling, slightly sudden pain
.—	tickle	rather annoying (5)

Ha

383	noise	sudden startle, alarm
—186, 222	whistle	startle more than noise
146, 106	arithmetic	slight anticipation, puzzled, helpless
177	" Cat o' nine tails "	hideous, disgust and horror
171	gun	fright, self disgust
143, 118, 45		
61, 80	threat of shock	excitement, slight anger
96	pin	annoyance, pain
82, 82	expectation	
61	electric fan	
41	goggles on	
34	" Solange "	horror, disgust
20	" all over "	relief
13	expectation before match	
7	gong	interest, rather pleasant
...	tickle	amused
...	asafoetida	neutral
...	match	slight fear
...	burn	slight pain
...	fan	rather pleasant

Han

151	whistle	sense of dread most intense
101	tickle	slight aversion
101	pin	almost fear
50	match	dreaded some
...	asafoetida	slightly unpleasant
...	burn	hurt
...	" all over "	expected more

Score: Per cent of mean	Stimulus	Response
		He
224	goggles	
134	noise	startling painful tension
153	match	some apprehension, hand sweated
122	pin	pain
89	tickle	amusement
30	burn	pain
22	asafoetida	slightly unpleasant
22	whistle	startled, slight momentary fear
...	"all over"	some relaxation
		Ho
451	after shot by *O*	
314	gun	very intense startle
248	shot by *O*	very unpleasant
243	after candy	expected?
243	whistle	awful, fear
207	soap	indifferent, then dread that it was dirty
189	gun expectation, shot by *O*	anxious (dread of loud noises)
171	formaldehyde	tiredness, strong associations
139	shock (problem)	didn't bother much
131, 63, 95	"Erl König"	rage, tense
125	whistle (2nd)	fear
121	noise	surprise and alarm
105	tickle	neutral
99	soap from fingers	
87	end "Cat o' nine tails"	
74, 42	"cattle"	anger, shrinking
71, 31	problem	
50	arithmetic	baffled feeling
50	arithmetic shock	didn't mind much
40	"all over"	
39, 27	"Cat o' nine tails"	slight shudder, disgust
37	quiz	chagrin, astonishment
37	"Amontillado"	enjoyed, tense
34	gong	unpleasant then pleasant
34	fan	
33	asafoetida	almost nauseating
27	burn	slight pain
22	blood	didn't bother
11	match	no fear
7	whistle (3rd)	absolute fear
		How
120	during quiz	embarrassment
100	formaldehyde	expected heat, odor didn't affect
80	gun	shock, not fear
...	exam. farce	slight disappointment
...	candy	expected unpleasant, relief
		Lu
206	shocks	dread
186	quiz	slight humiliation
160	goggles	
158	burn	pain, then relaxed

Score: Per cent of mean	Stimulus	Response
143	match	slightly unpleasant, tense
115	"all over"	relaxation
106	formaldehyde	didn't notice (anticipation)
93	exam. farce	satisfying
59	gun	decided surprise, no fear
57	noise	surprise, shock
43	whistle	surprise, shock, unpleasant
40	soap	surprise, unexpected
16	gong	slightly pleasant
13	after quiz	slight dread of next
...	tickle	slightly pleasant
...	asafoetida	slightly unpleasant
...	"Solange"	slight dread
...	fan	
...	"Cat o' nine tails"	slight disgust or anger
...	candy	satisfaction
...	problem	anticipation
		Me
278	gun	startled, fear, soon over
208	noise	slight startle
138, 127	shocks	dreaded very much
90	"Solange"	creepy, cold shivers
87	exam. farce	real relief
69	tickle	rather pleasant
63	arithmetic	enjoyed, slight disappointment at failure
63	after "Amontillado"	expectation
56	prick	pain
53	"Amontillado"	slight horror, dislike
34	quiz	nervous dread
28	blood	slight dread
...	gong	slight startle, slightly pleasant
...	"all over"	relaxed
...	asafoetida	slight aversion
...	whistle	startle
...	formaldehyde	
...	soap	aversion
...	candy	enjoyed
		Pa
509, 308, 135, 181	shocks	pain, severe apprehension
226	whistle	much surprised
164	burn	sharp pain, startled
123	match	very slight apprehension
107	after whistle	expectation
69	problem	slightly amused, then slightly apprehensive
49	beginning	expectation
42, 45	quiz	slightly annoyed (not real)
21	soap	rather pleasant
21	candy	very agreeable
19	goggles	
19	"all over"	relieved, relaxed
...	"Cat o' nine tails"	slightly unpleasant
...	formaldehyde	didn't get, expected shock
...	exam. farce	relaxed

Score: Per cent of mean	Stimulus	Response
		Pat
293, 30	gun expectation shot by *O*	intense fear
289	prick	shock or surprise
108	after "needn't shoot"	decided relief
90	blood	anticipation, fear slight
32, 26	"Erl König"	pathetic, sympathetic, very affective
15	"all over"	pleasant relief, relaxation
13	goggles on	
...	gong	indifferent
...	candy	very pleasant
		Se
267	tickle	pleasant amusement, then irritating
212	gun	shock and surprise, pain in ears, etc.
200	noise	attention elsewhere, slight chill
137	expectation	
118, 70	match	anticipation, no dread
110	whistle	startled, no fear, chill
47	expectation	
35	asafoetida	no reaction
31	goggles	
19	burn	pain
19	soap	not quite disgust
19	"all over"	
12	beginning	
		Sp
553	gun	almost panic, deafened ears
161	whistle	startled, sensation in stomach, tight throat
135	match	fear increasing acct. of expectancy
54	asafoetida	no especial response
47	tickle	no affectation
40	noise	slightly frightened, short duration
40	soap	slight disgust
33	burn	slight pain, less than expected
32	after "all over"	expected something more
14	goggles	slightly curious, thought could get no emotion
		Th
233	gun	fear, scared
147	gong	startled, solemn
107	after "cattle"	expectation
29, 47	"cattle"	interested, enjoyed
33	"all over"	
...	"Solange"	enjoyed all but end
...	"Old Refrain"	quite pleasant
		To
251, 115, 72	quiz	fear, confused
140	candy	expected pepper, surprise
53	formaldehyde	almost panic over quiz, associations
38	soap	fear of shock, enjoyed soap
28	"all over"	still tense, couldn't relax
...	exam. farce	elation, relaxed

Score: Per cent of mean	Stimulus	Response
		Tr
451	whistle	rather intense startle
126	gun	most unpleasant startle
110	soap	unpleasant, slightly nauseating
63	tickle	not very affective
63	expectation	
47	noise	startle, sudden, not fear
47	didn't burn	no excitement
31	expectation	continued expectancy throughout of electric shock in left hand
15	asafoetida	unpleasant
...	match	no excitement
...	"all over"	expectancy left
		Wa
382	gun	very startled, intense pain in ears and head
118, 368	whistle	decided start, pain in ears
81	expectation	
37	burn	slight sting
25	noise	very startled (in chest)
24	tickle	not very unpleasant, smiled then became irritated
24	match	didn't think it was lighted
18	asafoetida	not very affective
12	expectation	
4	soap	very repugnant, cringed
		Wag
233	pin	slight pain (feeling of security throughout)
106	at very end	
77	match	some apprehension
71	whistle	startle more than noise
70	asafoetida	just noticed (expectation)
41	burn	slight sting
...	noise	decided shock or startle
...	"all over"	relaxed
		Wi
210	match	expected burn, but no dread
120	gun	distinct fright, short duration
105	expectation	a good deal of apprehension before each stimulus, increased throughout
75	expectation	
45	burn	painful, tense muscles
45	"all over"	ready for more
...	noise	surprise
...	tickle	became pleasant
...	pin	no affectation
...	asafoetida	rather repugnant
...	whistle	startle or fright
...	soap	repugnant

Score: Per cent of mean	Stimulus	Response
		Wo
491	shock	startle, great horror or dread
351	gun	very frightened
166	goggles	dread of shock throughout
166	formaldehyde	expected shock, repugnant associations
148	gong	startle, then pleasant
134, 58	shocks and expectation of problem	very intense fear or dread of shock
105	goggles	apprehension
78	goggles	no affectation feelings
70	whistle	startle (not fright) (2nd)
58	tickle	startle, more than noise
46	noise	startle
43	soap	expected shock, fear, soap pleasant
35	asafoetida	pleasant relief
34	blindfold off	fear of shock again
23	"all over"	anticipation continued
23	prick	slight jump
23	"all over"	lost expectancy
17	exam. farce	elation, relaxed
15	"Solange"	rather pleasant, relaxed
13	candy	expected unpleasant, liked it
11	"Cat o' nine tails"	slight disgust and indignation
...	blood	no apprehension
...	fan	very slight startle
...	"all over"	really expected more
		Yo
—855	gun	greatest shock, intense muscular contraction
452	noise	startled
144	expectation	
67	expectation	
54	match	no anticipation
54	burn	fingers smarted
48	soap	slightly unpleasant
38	expectation	
9	expectation	
—250?	whistle	startled, piercing
...	tickle	pleasing
...	asafoetida	no affectation
...	"all over"	prepared for more

scored computed. These are listed in descending order of magnitude in Table II. An examination of Table II shows that the "startle" stimuli, the gun, the whistle, and the noise fall well at the top of the list. So also do the electric shocks which cause a "startle" reaction. The pin prick stimuli, which cause physical pain, are also high. The burn, however, is considerably lower than is the expectancy of it (match). The items at the bottom of the table seem to be either things which are very

slightly affective or which are conditioned on experience and may or may not be affective, depending on the *O*. From this we may assume that the fear stimuli which more generally elicit the psychogalvanic reflex are sudden intense stimuli which are described affectively as "startles." The small number of cases for each stimulus must not be overlooked, but the differences are great enough in the whole table to allow for the above interpretations.

A more significant treatment of the data is shown in Table III. Twelve emotions were listed and the scores for each emotion as reported by every *O* were combined and the arithmetic mean was taken for each emotion and for deflections which were reported as non-affective or neutral. The mean scores are again tabulated in order of descending magnitude. By far the greatest deflec-

TABLE II. *Descending order of deflections according to stimuli for all Os*

Score: % of mean	Stimuli	No. Cases
349	gun shot by *O*	2
256	expectation of shot (by *O*)	2
240	gun	12
208	shocks (problem)	8
167	whistle	16
122	prick	3
119	noise	15
112	arithmetic shock threats	12
110	pin	5
102	formaldehyde	7
99	shocks (arithmetic)	4
92	quiz	6
87	match	15
79	tickle	14
69	"Erl König"	5
64	expectations	23
63	"cattle"	4
53	burn	14
51	"Amontillado"	3
50	gong	7
50	goggles	17
47	"Cat o' nine tails"	7
43	soap	13
40	problem	10
36	asafoetida	16
35	blood	4
33	exam. farce	6
31	"Solange"	7
24	candy	7
18	"all over"	25
9	fan	4

Table III. *Descending order of magnitude of deflections according to reported emotions*

% Mean	Emotions	No. Cases
169	startle, surprise, or shock	53
123	fear	14
111	fear and apprehension combined	89
109	pain	19
108	apprehension dread, or expectancy	75
114	humiliation, embarrassment	3
80	anger, irritation	9
72	gruesome horror	5
60	disgust or repulsion	14
45	relief	15
35	neutral "didn't bother"	23
26	pleasant	25
14	slightly unpleasant	6

tions are occasioned by stimuli causing emotions which are described as startle, surprise, or shock, the mean score being 169. The next greatest reactions with a mean score of 123 were described as fear. This, according to reports, seems to be very much the same as apprehension or dread, which, with a mean score of 108, is classed fifth. If they are considered together they rank third. These differences, however, are slight and are of little significance. The important fact is that the " startle " emotions appear much greater than do the apprehensive fears in so far as they are measured by the psychogalvanic reflex. There is no absolute standard by which it can be determined whether the stimuli used for production of apprehensive fears were sufficiently intense to be comparable to those causing startle. An examination of the introspections may give some cue to this.

On the whole, the nature of the stimuli were such that startles and fears would tend to be more intense than other emotions, so the lower end of this table can not be accepted without reservation. Other studies, however, have reported that fear stimuli cause the greatest deflections.

Situations which were reported as not affective show in this table a considerable amount of deflection, averaging 35 per cent.[6] There are two factors which seem important in causing this. The whole situation was calculated to induce fear, and these reactions

[6] Syz (99) has shown that the subject's report of the presence or absence of an emotion in response to association words, is often unreliable.

may be due to expectation of something so much worse than the actual stimulus that it in itself produced no conscious emotion but came as a relief from the apprehension caused by the uncertainty of what would happen. In some cases *O* may not have been con- what would happen. In some cases *O* may not have been conscious of an emotional reaction because through training he had acquired the attitude of not being affected by certain things. Many of the reports were worded in such terms as "that wasn't bad" and "that didn't bother me much," indicating a somewhat defensive attitude toward the situation.

A few other general tendencies are found by casual inspection of the records themselves. The tracings in Plate III show that most of the stimuli of a "startle" nature cause a very abrupt change of resistance. The whistle in A, the noise and the tickle in B, and the whistles in C are examples of this. On the other hand, "anticipation" or "expectancy" deflections are as a rule more gradual. This is noticeable in the shock threat in D and the match or expectation of burn in F. An inspection of all of the records shows that of forty-eight deflections which could be characterized as very abrupt changes, thirty-six, or 75 per cent., of them were caused by stimuli which were described as shocks or startles. This is not complete agreement, however, and many of the emotions described as startles are not accompanied by a reflex of this character. On the other hand, abrupt changes which were caused by other than "startle" stimuli usually occurred when *O* was excited and tensely expecting something to happen. It indicates that on the whole "startle" emotions are much more sudden than apprehension emotions. The latter would appear to accumulate gradually with more complete realization of the situation.

During a sitting, the level of resistance usually shifts continuously in one direction or the other. Comparisons of the records with introspective reports show that in general the direction of shift coincides with the amount of excitement or apprehension felt by *O* throughout the test period. Readings showing continued shifting to the left as in B, C, D, and F, in Plate III,

indicate a lowered resistance. The psychogalvanic reflex is always in the nature of a lowered resistance. The reports of these *O*s correspond with the inference that the affective tone was heightened throughout the experiment. *Se* reported for the experiment from which sample B was taken, a feeling of anticipation which lasted until the end. *Tr* for example C was continuously expecting an electric shock in his left hand where the electrodes were attached. *By* in D felt much more excited and wakeful after the shock than before, and in E, though feeling tense during the whole reading, was too fatigued to keep her attention long on the emotional situations, and they were of relatively short duration. In F, *Wi* also reported a good deal of apprehension before each stimulus. This apprehension became greater toward the end. For G, which shows the opposite trend, *He* made an attempt to control his general attitude as he would under hospital conditions, and found that he became more normal toward the end. *How,* whose curve was similar to that of *He,* reported no intense emotions and appeared to be very little excited. Here again there are exceptions to the tendency. Some *O*s who reported a continued emotional state did not show it in the record. This may be due to individual differences in skin resistance and the amount of polarization in the skin, or, as indicated in sample E, to bodily fatigue. During the reading of stories, with a few exceptions, the resistance level gradually increased. Very few of the *O*s felt apprehension during these stories but reported instead repulsion, horror, and feelings of gruesomeness which were rather depressing, or else merely a calm relaxed condition.

A number of rest curves were made with the *O*s assured that no stimuli would be given. These were taken over 5 and 10 minute periods and they invariably resulted in a general shift to the right. It seems reasonable to assume, then, that continued emotional tone in fear situations is evidenced by a general lowering of resistance.

The reactions of *O Ca* were exactly opposite from those of all the other *O*s (Plate IIIA). If reversed, the deflections are

typical deflections to the stimuli given. As *Ca* could not be secured for a second experiment, no check could be made on the reactions. It is offered here as an exception which without further evidence is of little significance.

Variations in resistance on different days and at different times of the day for the same *O* have been noted by most of the investigators of the psychogalvanic reflex. Such experiments as those of Waller (109) and of Farmer and Chambers (27) show periodic changes during a twenty-four-hour day. These were thought to be due in part to variations in fatigued and rested conditions of the *Os*. We made a single observation with *By* comparing reactions while in a rested and a fatigued condition. Sections of the results of this trial are shown in samples D and E of Plate III. They were obtained during one evening between 7:30 and 10:00 o'clock. For the rested experiment (D), *O* and *E* carried on a pleasant conversation while looking at prints of oil paintings, and eating candy. *O* then lay relaxed in a comfortable position for about five minutes after which she went into the experimenting room and experienced a fearful situation while connected in series with the galvanometer. The stimuli were first a threat of a strong electric shock, then *E* said "get ready," and after a few seconds the shock was given. For the fatigue series *O* did vigorous setting-up exercises until physically tired, and then was subjected to a difficult oral quiz and made to feel very discouraged by her inability to answer the questions satisfactorily. Galvanometer readings were then taken for the same stimuli. The results of this are shown in E (Plate III).

The bridge was balanced at exactly the same resistance for both experiments. The "rested" series shows an expectancy curve which started with the threat of the shock and continued during the whole experiment. The reaction to the shock itself was rather small and is not distinct from the general expectancy trend. *O* reported with the threat of shock anticipation and slight dread with muscular tenseness just above the knees and twitching of the hand where the shock was to be applied. When

told to get ready she became more tense and felt a decided fear of the shock, with involuntary twitchings spread over the whole body. The shock was painful and caused an involuntary reflex jump of the whole body with quick intake of breath. Afterward she calmed down gradually but felt much more wakeful than before.

The fatigue series shows no reaction to the threats but a strong reflex to the shock. *O* reports for this series fatigue and general tenseness with inability to relax. With the threat there was a slight anticipatory dread, but the muscles which were already tense, did not seem to change. With the warning to get ready, however, fear was experienced and the muscles tightened. The shock caused a reflex jump and was more painful than before. It caused a sharp pain in the head about the temples which lasted some time. The fatigued condition seemed to *O* to make it hard to attend to the stimuli, and the emotions were not continued but soon left. This last statement seems to account for the differences between the two records. Anticipation was lacking on account of fatigue and failure to attend for long to the emotional situation. For this reason too, the resistance did not remain lowered as in the rested experiment.

Relation of the Breathing Changes to the Psychogalvanic Reflex. Because in describing startles and alarms the *O*s mentioned breathing changes far more frequently than they did in other emotions we decided to make a pneumographic record with the galvanic record to determine if the breathing changes were more frequent with startle reactions. The breathing curves on the records shown in Plate III are typical of all of the records. The changes in breathing show some correspondence with the psychogalvanic reflex, irregularities either in rate or in depth of breathing or both occurring with most affective stimuli. This, however, is not invariable. Breathing disturbances occur where there is no galvanic deflection, and deflections occur with no breathing change. The changes occasioned by anticipatory emotions are not unlike those caused by startles, and they are just as prevalent. Types of breathing changes seem to have no

influence on the galvanic curve, nor do they show in themselves patterns which are characteristic of different emotions.

Introspections of fear. Although the *O*s were not introspectively trained, their reports show distinctions which would indicate some fundamental differences in kinds of fear. This is evident from reading the reports when they are grouped into startles, apprehensions, and pains, as below.

Fear proper

Ba. *shock*—caused jump and contraction but no emotion.
whistle—slightly stunned, inhibition of everything, then motion started in thorax and moved down, similar to dream of falling but not so intense; sort of a fear.
noise—caught unawares, for two seconds had a real emotion, "got sore," turned toward the sound and caught breath.
gong—slight startle felt in chest, moved down a little but didn't last long.
gun—stunned at first and real fear, when realized what it was the whole affective condition gradually disappeared.
gun shot by O—scared, surprise complete (had thought it not loaded) jumped.

Bl. *noise*—not very intense because wasn't expecting it. There was a sense of muscular tightness around the ears.
gun—shock, general muscular contraction and drawing together, no fear but intense surprise or startle. Gradually relaxed when realized what it was. Noticed heart beat faster, having slowed with the shock.
shock—surprised or startled, muscles of arms and chest contracted, quick inspiration with exclamation of pain, fingers were damp with perspiration.
whistle—very startling, muscular contraction especially in arms and chest, then heart began to beat faster.
shock—jerked away, muscular contraction especially in arms and chest.

Be. *tickle*—start, surprise, momentary, became disgusted with self for starting.
whistle—very startling but not very disagreeable in itself, the unpleasant part was the surprise, this left rapidly, distinct start.

Ch. *noise*—very intense unpleasant shock, sinking feeling in stomach and chest, tingling all over even in feet, change in heart action, muscular contraction.
whistle—startled, cringed from it, the sound filled consciousness entirely blotting out all else, felt weak afterward; relaxation was gradual.
gun—almost terror for a moment; whole body tingled; sinking feeling in stomach; accelerated heart beat; was hard to relax afterward; felt trembly.

Go. *noise*—"go to pieces feeling" for a second then felt silly; tightening up and then relaxing, mostly in shoulders.

He. *noise*—startling; jerking of lower body muscles and in breath; painful sensation through lower limbs; the surprise was immediate, but was offset by the fact that nothing harmful happened.
whistle—startled, slight very momentary fear; jerking in upper body and painful sensation in this portion of the body.

gun—very startling; general contraction of all body muscles; deafened for several minutes; organic change somewhat like dropping sensation.

Ha. *gun*—jumped; felt fright; legs contracted; then disgust at self.

Ho. *noise*—jumped; general muscular contraction; surprise and alarm very intense.

gun—very intense; took breath; was hard to get breath again; the noise rang in ears and movements of *E* seemed far away; wasn't fear but decided startle.

whistle—fear; caught breath; knees shook; goose flesh all over whole system; felt tense and couldn't relax.

whistle—(repeat) absolute fear; jumped all over; couldn't get breath again; weighed very heavy on chest; was like waking up after a nightmare. (*O* reports intense fear of all loud noises.)

How. *noise*—distinct shock; for an instant fear; reflex muscular contraction which was general; noticed especially in thoracic contraction and catch of heart action.

gun—was expecting it; shock, not fear; physical reaction to suddenness of explosion; tenseness and sharp attention.

Lu. *gun*—start; very decided surprise; head moved forward and hands jerked; afterward there was a tingling in the fingers; the noise was not especially unpleasant in itself; felt tenseness in chest, with breathing disturbances; heart was beating harder and more rapidly.

Pa. *noise*—started violently; fright didn't last long; prickling in extremities; tension in diaphragm; visual image of a burst of light.

whistle—much surprised; movement of whole trunk away from; a wave of unpleasant affective tone starting at diaphragm flowed down and out to extremities where felt a tingling.

Se. *whistle*—startled but no fear; chill sensations in skin; sinking or heaviness in stomach; ringing in ears even to pain.

Th. *gun*—very surprised; scared just for a moment because still thinking of Solange; jumped; tenseness all over body especially in back of legs; reaction purely physical and not connected with mental.

Tr. *noise*—sudden startle reflex jump; not fear.

Wag. *whistle*—startle; made hair stand on end; goose flesh on left side; catch of breath; feeling like tearing cloth; teeth and nerves on edge.

Wo. *shock*—startle; hands perspired; great muscular contraction all over body.

Apprehension

Bl. *match*—apprehension, muscular tension mostly in right fingers, hand and arm where attention was centered.

general—when *E* came close, expected something to happen; heart beat much more apparent at these times.

problem—anxiety for fear of failure, tension, concentrated on it. On learning would receive 5 shocks determined wouldn't believe it, but was rather apprehensive.

threat of shock—after first shock dreaded others; braced self to hold hand still; contraction of all muscles, especially upper trunk; mixed apprehension and dread as in a situation when you don't know what will happen.

threat of shock (arithmetic)—apprehension; dreaded to give answer.

Ch. *match*—feeling of expectance and slight nervousness; increased as grew warmer; the expectation was worse than the realization.

general—apprehension became gradually more intense; noticed muscular tenseness.

Go. *match*—hated to take it, dread; wished could see it; threw it down before it actually burned; tried to find some way to get out of holding it.

Ha. *threat of shock (arithmetic)*—helpless; baffled; excitement and increased interest; dismay at failure; slight dread of shock.

Ho. *problem*—baffled feeling with attempts to make estimates; when E tinkered with apparatus of shock very decided reaction, and muscles grew tense.

arithmetic—baffled feeling; felt couldn't do them anyway.

Lu. *match*—slight apprehension of burn with slight tenseness and a tendency toward organic sensations; but was concentrating finger in order to drop it quickly.

problem—couldn't figure; finally took wild guess; dread increased as the shocks continued; when got to 4 or 5 seemed like a long way to 13; but after that they seemed to go rather quickly; took attitude of resignation; breathnig seemed to go by jerks.

Me. *blood*—wondered why this was being done; pulling back of hand; slight dread.

quiz—felt a little nervous; a falling away in the chest; desire to be out of it; constantly slight afraid of what would come next; trepidation.

Pa. *problem*—slight apprehension until after shock; muscles tense and began to wonder if could stand 7; tension in arms and legs and in abdominal region.

general—apprehensive when E moved near; slight tenseness; keyed up; was expecting a shock.

Pat. *expectation of gun (shot by O)*—intense fear; hand got very clammy and rather shaky; pressure in chest; sinking in pit of stomach; tensing of muscles.

Ru. *match*—anticipation partly fear; tendency to drop it; and dropped before really burned much.

problem—the anticipation was definitely fear; mostly of the shock; caused more close attention.

arithmetic—dreadful; afraid to try figuring without pencil; nervous; sure couldn't get right answers so really afraid; held breath a great deal; went through more mental anguish than in any other experiment.

To. *quiz*—fear; chill passed over; felt trembly all over; mind seemed confused.

expectation after quiz—almost in panic; was fussed at failure.

Wo. *problem*—dread of shock when knew couldn't save self; intense horror or dread of the shocks.

Pain

Ba. *burn*—very little pain, jerk, then vanished.

pin—sharp feeling; not emotion but pain; unpleasant; slight muscular tension in right arm; tendency to draw away.

Bl. *pin*—annoyance; some pain; surprise; involuntary withdrawal of hand.

burn—slight pain; no emotion; withdrawal.

He. *pin*—largely sensational pain; no apprehension; felt secure under conditions of experiment.

Lu. *shocks*—stinging pain and unpleasant.

Me. *prick*—pain.

Pa. *burn*—sharp pain; started and jerked hand away.

The introspections showed that with startles attention seems to be directed more to the intense physical reactions. There

were also frequent references to tingling of body and various parts of the body. This is probably evidence that the muscular contractions were intense and sudden. On the other hand reports of dreads and apprehensions had less of the organic in them and more of the ideational. Many *O*s definitely stated that the startles were not fears but were purely physical reactions. In anticipation the physical reaction when it was present developed more slowly. It depended on a perception of the situation as fearful or harmful.

The pain reactions were very similar to the startles. Perhaps the main difference here is that they are occasioned in a different sense department than are the startles caused by noises. They might better be classed under the startles.

Comparison between introspective and psychogalvanic results. The psychogalvanic reflex which is characteristic of "startle" fears is a very abrupt rise which is ordinarily of greater magnitude than the reflexes caused by other emotions. In correspondence with this the introspective reports indicate sudden and intense organic and muscular reactions to such fears to a far greater extent than to other emotions. It may be possible that the absence of fear in startles is only apparent, because recognition of the situation as harmless comes so soon under laboratory conditions, but if fear is apprehension of a harmful event, then startle is quite possibly qualitatively different. These introspective distinctions agree with the results of the psychogalvanic reflex in the two types of "fear." Introspectively startle fears are very short-lived, more so than the duration of the reflex would indicate, except for the more lasting physical effect. This is apparently because of the perception of their cause and their harmlessness. Apprehensive fears, because of the longer duration, and the perceptual element of realization of imminent danger seem to be qualitatively different.[7] The only physiological difference which could be inferred from this study is the sudden-

[7] Watson considers only sudden fears of a startle nature. His experiments point to the possibility that all apprehensive fears (including fear of snakes and the like) are learned and depend on perceptions built up through conditioning.

ness of the reaction of the startles as opposed to the gradual reactions of apprehensions.

Conclusions. 1. The most intense psychogalvanic reflex deflections occurred in response to stimuli which were described as startles, shocks, and alarms. Whatever the physical reaction which occasions the change in skin resistance, it seems to be greater for the most part in response to sudden unexpected stimuli. This is shown to be true both when the reactions are classed according to stimuli, and to introspective reports of the emotion experienced.

2. The gradual change of resistance which occurs during a single sitting is affected by the state of apprehension or ease of the *O*. Continued excitement and apprehension cause the general bodily resistance to decrease independently of the separate deflections. When no expectation is present bodily resistance tends to increase. In connection with this there is some slight evidence based on a single experiment with *By,* that body resistance increases rather than decreases when *O* is fatigued, even under considerable excitement.

3. Reflexes which are caused by startling stimuli are temporarily more abrupt than those caused by anticipatory fears. The response to the startle is complete from the beginning, while the apprehension of danger grows comparatively slowly with perception and realization of the situation. This is not invariably true, but is predominantly so. The exceptions usually occur when the *O* is in a tense and excited condition.

4. Introspectively startles are differentiated from apprehensive fears in a way which corresponds with the temporal character of the reflex. The reports indicate sudden and intense physical reactions for startles with almost immediate realization and subsequent calming and relaxation. Anticipations and dreads are described more frequently in mental terms denoting expectancy and increasing uneasiness. The physical reaction, however, is present in the anticipations and is often described, but does not appear so prominently as in the startles.

5. Breathing changes usually occurred with emotional reac-

tions, but these changes were not differentiated in nature with regard to different kinds of emotions.

6. Startles appear to be textually different from apprehensions because they are of very short duration, and because they are reflex rather than perceptual in nature. In startles the perception occurs after the reflex response, and when the sudden stimulus is not accompanied by danger the perception of the situation relieves the mental anguish, leaving only the sensations of the physical reaction. In apprehensions the danger usually is perceived more slowly and continues over a comparatively long space of time.

7. This study indicates, both quantitatively and introspectively, that there are at least two types of fear, namely startle, and apprehension. They are distinguished by the difference in the immediateness of the reaction, and in its duration.

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THE APPLICATION OF PHI-PHENOMENA TO BEATS[1]

by

INGVALD B. HAUGE

Historical introduction; statement of problem; apparatus; technique; results; conclusions; bibliography.

Historical introduction. Some unsolved situations in the experimental work on the phi-phenomenon by *Wertheimer* (5), *Dimmick* (3), and *Higginson* (5) and an investigation of the beating complex by *Eberhardt* (4) have lead the experimenter to make a further investigation regarding the nature of the beating complex and the application of the phi-phenomenon to beats. The investigation has as a matter of fact two sources: the experimental work (1) on the phi-phenomenon and (2) on the beating tone complex.

Wertheimer defines the phi-phenomenon as the "experience of movement between two objects, where the movement appears after the presentation of the second figure but is usually conjoined with the first and where nothing in the object corresponds to the experience of movement." (6) *Higginson* views the phi-phenomenon in the visual field as "the visual apprehension of movement" and adds that it is "a form of abstractive perception determined by stimulus, receptor and central organs, the degree of abstraction depending on the relative contributions of environmental and organic factors." (7)

A review of the investigations made on the beating complex shows that a beating intertone with a pitch close to that of the lower primary has been heard (8). Some workers in this field report that both primaries can be heard in addition to the beating intertone (9). The effect upon the beating complex of varying the relative intensities at which the primary tones are sounded

[1] This study was made under the direction of Dr. Christian A. Ruckmick.

has been investigated by *Eberhardt.* In this study Eberhardt used two primary tones at 8 d.v. difference. When the primary tones were of unequal intensities all of her *O*s agreed in hearing one beating tone or intertone. When the intensities of the two primary tones were made equal or nearly so the *O*s heard two tones which were beating. They were not the primary tones. The pitch of one of these tones was near the pitch of the louder primary while the pitch of the other tone was from 2–5 d.v. less than the louder, which in some cases would bring it close in pitch to the lower tone. *Eberhardt* reports that in nearly all cases the beating tones were heard simultaneously with the primary tones. In these experiments Stumpf was one of the *O*s (4).

Up to the present, then, no attempt has been made to determine what happens to the beating intertone while the intensity of one primary tone is decreased at the same rate as the intensity of the other primary tone is being increased. There are no reports which indicate a thorough and careful analysis of the beating intertone when the intensities of the two primary tones are equal, when they are unequal or when the intensities are being varied in certain fixed relations to each other.

Statement of problem. It is the purpose of this research to investigate the possibility of interpreting beats in terms of the phi-phenomenon. This involves an analysis of the factors surrounding the beating complex. The problem involves the following questions. Is there apparent auditory movement of beats from one primary at its pitch level to the other primary at its level of pitch? Are the beats ascribable to a subjective intertone? If so, does the pitch of this intertone change? Is the change of pitch in the beating intertone gradual and continuous regardless of outside conditions? Are there situations under which more than one intertone can be heard?

Eberhardt contends that two beating intertones are heard when the two primary tones are sounding with equal intensities and that only one intertone with a pitch near the louder primary is heard when both primaries are sounding at unequal intensities (4). *Bentley* states that only a third beating intertone inter-

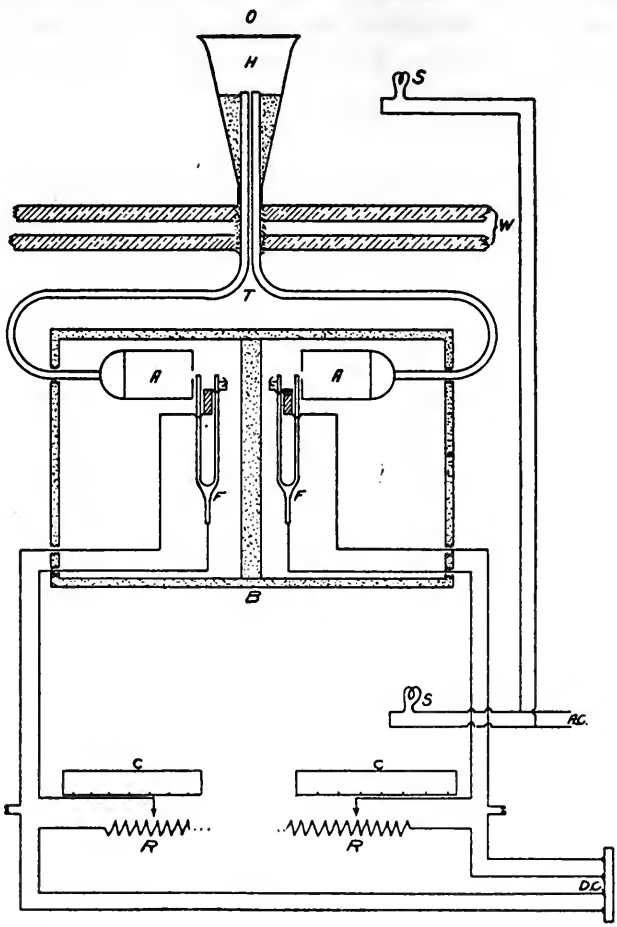
mediately situated as regards pitch can be heard (1). *Titchener* quotes from *Stumpf* that the two primaries can be heard in addition to an intertone whose pitch is somewhat nearer the lower than the higher primary tone (9). *Broadhouse* writes that beats are most distinct when the intensities of the two primaries are equal (2).

Some of the more important general conditions under which observations of the beating complex were made are the following. Both forks were allowed to vibrate at maximum and minimum intensities respectively. Both forks were allowed to vibrate at medium intensities. The higher fork's intensity of vibration was decreased very gradually from maximum to minimum intensity during which time the lower fork's intensity of vibration was increased at the same rate as the higher fork's intensity was decreased.

Apparatus. Quotations from *Stumpf* by *Titchener* show that the most easily recognized beating intertone is produced when the difference in vibration rate of the two forks is not less than 6 d.v. or more than 22 d.v.(10). The vibration rate of the two forks was in the vicinity of 256 d.v. In a series of preliminary experiments with tuning forks whose difference in vibration ranged from 1 d.v. to 30 d.v. the writer found that the most easily distinguishable intertone was secured with a vibration difference of 12 d.v.. Consequently two electrically driven tuning forks of 256 d.v. and 244 d.v. respectively were selected for the experiments. A rheostat of suitable size was inserted into the circuit of each fork. The forks were electrically driven with separate direct currents. By means of these rheostats the respective intensities of the two primaries were controlled by regulating the amount of current admitted to each fork. One primary could be increased in intensity while the other primary was decreased or both primaries could be either increased or decreased at the same rate and at the same time. The rate of such changes could be made gradual or rapid. Pulley connections between the two rheostats made these controls possible.

On the centimeter scale of each rheostat was marked the points

at which the primaries sounded at maximum, minimum and medium intensities. In each of these three positions the intensities of the two primaries were subjectively judged to be equal. A condenser inserted in the circuit of the higher primary reduced the spark where it made contact with the fork. Each tuning fork was placed in a highly sound-proof compartment of a box with the properly adjusted resonators attached to each fork. The



Legend: **C** — centimeter scale; **S** — signal light; **O** — observer's position; **H** — horn; **W** — wall; **A** — adjustable resonators; **F** — forks; **R** — rheostats; **T** — tubes; **B** — box.

room was sound-proof. Rubber tubes connected with the resonators conducted the tones of the forks into an adjacent sound-proof room through a closely fitting opening in the wall. Each tube was three feet in length with a diameter of 1″. The tubes ended in a horn large enough to cover the observer's ear. "Ozite" packed around the tubes in the horn reduced the reflection of the tones which came from the forks. The arrangement eliminated the sounds produced by the contact points of the forks. By means of electric light signals *E* signalled the *O* when to begin and when to discontinue observation and *O* signalled *E* in situations where *O* was required to report when he noted certain changes. A silent metronome was used to control the time element. The *O*s used in this research were selected with special reference to their ability in pitch and intensity discrimination. The number of *O*s was reduced to four. Three were graduate students in philosophy and psychology and one was an undergraduate. The following diagram shows the essentials of the apparatus used.

Technique. The four *O*s were given preliminary training extending over a period of two months. Its purpose was to give the *O*s opportunity to describe in non-psychological terms the auditory sensations which they received from a beating complex. A further purpose was to train the *O*s to distinguish pitch changes from intensity changes of the two primaries when they were sounding successively and to distinguish the same changes with respect to the intertone produced when the two primaries were sounding simultaneously. Two forks about 12 d.v. apart were used. Reports secured in the preliminary training suggested further methods of procedure in training the observers. The preliminary training was used as a means of eliminating the *O*s who did not show sufficient ability in pitch and intensity discrimination and ability to center and keep their attention on the beating complex.

In order to get the most unbiased reports from the *O*s the following general precautions were taken. All statements or phrases which might suggest to the *O* what changes could be

heard were eliminated from the instructions until the *O* had used such statements in his reports. The four *O*s were also instructed not to discuss their reports with one another. Chance distributions of the various types of trials were always used. At the beginning of each sitting the *O* was given a warming-up period. Frequent periods of rest were given in order to eliminate auditory fatigue.

The data secured in the preliminary training was used as the basis for devising the procedure used in Part I and Part II of the experiment. The prime purpose of Part I was to get descriptive data from each *O* in the form of written accounts, graphs, and diagrams. The purpose of Part II was to determine the reliability of the reports made in Part I. To do this various systems of checking were used. Space does not permit the writer to give the details of the procedure in each series or the instructions for each series in Part I and Part II. Accepted methods of determining the reliability of the reports given under the procedure in Part I were used.

Results. Only the most significant facts brought out in the reports follows. In the preliminary training T gave the following significant description of the beating complex: "I hear a rapid rise and fall in intensity which I would characterize as a beat. . . . A tone accompanies these beats. . . . The pitch of this tone is somewhere between the two primaries which I heard sounding separately at the beginning of the trial." In Part I whenever the upper primary was sounding more intensely than the lower primary all the *O*s reported consistently as follows. An intertone carried the beat; its pitch was close to that of the upper primary; its intensity was almost equal to that of the upper primary when it was sounding at its maximum alone. When the intensities of the two primaries were reversed, the *O*s reported that the pitch of the intertone was close to that of the lower primary and that the beats were less distinct and pronounced.

When the two primaries were sounding at medium and equal intensities T and A reported that they heard two beating inter-

tones. The description given by T is typical: " Instead of one intertone I heard two beating intertones; the pitch of one of them was close to the pitch of the upper primary while the pitch of the other intertone was near that of the lower primary; both of them carried a beat. Their intensities were equal and about half way between the maximum and minimum intensities of the upper primary when it sounds alone." It is to be noted that there was nothing in the instructions to the *O*s which could even suggest the possibility of hearing two beating intertones.

When the intensity of the upper primary was decreased very gradually from maximum to minimum intensity at the same time as the lower primary was increased from minimum to maximum intensity, the following typical description was given by one of the most reliable *O*s. The procedure of Part II shows its reliability to be beyond questioning. "At first I heard a high beating intertone with a strong intensity, which intensity decreased gradually; its pitch was close to that of the upper primary. After an interval of several seconds I began to hear occasional breaks in the even continuous pitch of the high beating intertone as if another lower beating tone was trying to break in. It seemed as if the higher was struggling to maintain its dominance. Gradually a lower beating intertone, whose pitch was close to that of the lower primary, could be heard sounding at the same time as the high pitched intertone. Their intensities were equal and medium. I continued to hear these for a few seconds. Then the higher beating intertone began to be heard less clearly; it was a period of confusion where the lower beating tone seemed to be gaining dominance. Finally the lower beating intertone began to sound continuously without any interruptions by the high intertone." When the procedure was reversed the following representative description was given: "At first I heard the low pitched beating intertone sounding with an intensity above medium. After a few seconds the high pitched intertone began to be heard intermittently as the intensity of the low intertone was decreasing. Finally I heard both the high and the low pitched intertones sounding simultaneously at a medium inten-

sity. After a few seconds there was a period of confusion; the low beating intertone was beginning to disappear while the high intertone was becoming dominant. Shortly before the end of the trial I could hear the high pitched intertone sounding continuously."

A brief explanation of the method used to determine the reliability of the above descriptions follows. When the intensity of the upper primary was decreased very gradually from maximum to minimum intensity at the same time as the lower primary was increased from minimum to maximum intensity by moving the sliding contact of the higher primary's rheostat downward on the centimeter scale from 0 to 30 cm, the *O* was instructed to signal (1) when the "low beating intertone begins to be heard," (2) when "both beating intertones are beginning to be heard clearly," (3) when there is "the beginning of a period of confusion where the higher beating intertone is disappearing," and (4) when "only the lower beating intertone is heard." When the above procedure was reversed by moving the sliding contact up the centimeter scale, the *O* was instructed to signal (1) when the "higher beating intertone begins to be heard," (2) when "both beating intertones are beginning to be heard clearly," (3) when there is "the beginning of the period of confusion where the lower beating intertone is beginning to disappear," and (4) when "only the higher beating intertone is heard." This fourth change in the beating complex when the procedure was reversed corresponds to a point just before the first change in the beating complex under the former procedure. The third, second, and first change under the latter procedure correspond in a similar manner to the second, third, and fourth changes respectively of the former procedure. Thirty reports as to when each of the above eight changes occurred were made. The first change in the former procedure and the fourth change in the latter procedure were studied together according to a chance distribution of trials. In each trial the location of the rheostat's sliding contact at the beginning of the trial was varied

according to a chance distribution. The same procedure was used for the other changes.

Chart I gives the averages of the points at which *O* reported each of the four changes to occur when the sliding contact was moved down the centimeter scale from 0 to 30 and when it was moved up the centimeter scale from 30 to 0. Data secured from two other *O*s are included in Table I.

The reports of the *O*s show that two beating intertones were heard sounding simultaneously during the time when the inten-

TABLE I. *Rheostat scale readings.*

Capital letters indicate the *O*s. The numerals which follow the capital letters indicate the respective changes described by each *O*. The standard deviation of each average is given in parentheses. Scale has 30 cm. units.

Descending	Ascending	Average of ascending and descending
6.71 K–1 (.699)	3.68 T–1 (.960)	5.74 T–1
7.81 T–1 (.869)	6.13 K–1 (.593)	6.42 K–1
10.76 K–1$_a$ (.844)	8.15 K–1$_a$ (.859)	9.45 K–1$_a$
12.96 A–1$_a$ (1.174)	10.86 A–1$_a$ (1.275)	11.90 A–1$_a$
14.73 T–2 (.893)	13.46 T–2 (7.28)	14.09 T–2
15.85 K–2 (.577)	13.85 K–2 (.447)	14.85 K–2
16.72 A–2 (1.118)	14.92 A–2 (1.014)	15.82 A–2
20.85 A–3 (.670)	18.15 A–3 (1.802)	19.50 A–3
21.51 T–3 (.746)	18.25 K–3 (.824)	20.25 K–3
22.25 K–3 (.625)	20.48 T–3 (.547)	20.99 T–3
26.46 T–4 (.553)	24.05 T–4 (.517)	25.25 T–4

sities of the two primaries were equal or nearly equal. The pitch of one of the beating intertones was reported to be near that of the lower primary while the pitch of the other beating intertone was placed near that of the upper primary. Their intensities were medium and equal. During the first part of the trial only the higher beating intertone was heard. Just before the lower beating intertone was heard distinctly and continuously there was a period during which it was heard intermittently. Toward the end of the period during which both intertones were heard with equal distinctness, the higher intertone was heard only intermittently while the lower beating intertone was beginning to sound more intensely. At the very end of the trial only the low intertone was heard.

Conclusions. On the basis of the descriptions given by four *O*s under the conditions of this research the writer believes that

the phi-phenomenon is not applicable to the perception of beats. There is no direct perception of auditory movement on the part of beats in terms of pitch. It was only in the preliminary training that perception of movement on the part of beats with their accompanying intertone from one primary to the other in terms of pitch was reported. For this reason the writer believes that the perception of movement is largely a matter of inference. It is similar to the experience in vision where we infer that an object has moved because its position has changed while we were looking at some other object.

When two pure tones with a pitch difference of about 12 d.v. are sounding at unequal intensities, one intertone is heard whose pitch is near the pitch of the tone which is sounding with the greater intensity. When the intensities of such tones are equal or nearly equal, two intertones are heard. The pitch of one of the intertones is near the pitch of one of the pure tones while the pitch of the other intertone is near the pitch of the other pure tone.

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AN EXPERIMENTAL CRITIQUE OF THE SEASHORE CONSONANCE TEST[1]

by

DELIA LOUISE LARSON

Review of theoretical studies; review of experimental studies; outline of problem; experimental procedure; discussion of results; gross comparisons with large groups, relatively unselected; gross comparisons with small groups, selected on the basis of musical training; analysis of intervals; conclusions; critical comments; explanation of tables; tables; questionnaire and correlations; bibliography.

Review of theoretical studies. Theoretically, consonance has been regarded mainly from four points of view: (1) the physical factors involved, such as relative rate, form, and coincidence of sound-vibrations, together with their secondary effects, such as beats and difference tones; (2) the physiological factors, including theories based primarily on factors that are either just below the conscious level or that involve the physiological mechanism of hearing; (3) the psychological factors, including theories based on actual experience arising from the presentation complex; and (4) the genetic factors, involving all theories which use natural association or habit as a basis in explaining consonance.

Helmholtz (8, pp. 182–197) presents one of the most widely recognized physical theories. He attributes the cause of dissonance and consonance to the relative presence or absence of beats among the partials in a fusion of different tonal qualities. A similar theory was advanced by *Krueger* (9, pp. 294–411) who stresses difference tones rather than partials.

The physiological or subconscious aspect was introduced by *Lipps* (11, pp. 115ff) who assumes "micropsychic" rhythms in his explanation of fusion. Unconscious rhythmical processes cause a fusion of tones when the rhythms are compatible and a lack of fusion when they are not compatible.

[1] This study was made under the direction of Dr. Christian A. Ruckmick.

Another theory which looks for its explanation rather toward the mechanism within the ear was advanced by *Ebbinghaus* (**19**, p. 316). It is based on Mach's hypothesis which assumes that two complex tones are more consonant, the greater the number of basilar fibers stimulated in common.

In the psychological group, we find *Stumpf* (**33**, pp. 91–107) making fusion the criterion for consonance. Fusion is an " immanent relation " giving rise to an unanalyzable uniformity of perception, conditioned by mental as well as physical factors.

Watt (**37**, pp. 53–73) also analyzes consonance in terms of fusion, describing fusion as a balance among the tonal masses of the fusing components as appearing in experience.

Titchener (**35**, pp. 336–337) likewise interprets consonance chiefly in mental terms, with incidental reference to mathematics and physics.

> " Fusion depends upon distance on tonal scale, absolute and relative intensity, spatial separation, duration of clang, attention, practice, and fatigue, expectation and habituation, memory."

He recognizes the tendency to confuse pleasantness and unpleasantness with higher and lower degrees of fusion.

Wundt (**39**, p. 452), in his psychological theory of fusion, recognizes three criteria of consonance: (1) relatively narrow unity of fusion, (2) distinctness of tonal fusion, and (3) dominating tonal element.

Seashore (**28**, p. 145) emphasizes the complex nature of the phenomena of tonal fusion. Consonance cannot be judged in terms of agreeableness as this is a changeable criterion. The decision must be based upon a cognitive judgment rather than upon a mere feeling of pleasantness or unpleasantness. Blending, smoothness, and purity are considered in judging consonance. For dissonance, the factors are disagreement, roughness, and richness.

Likewise, *Külpe* (**10**, pp. 282–283) stresses the complexity of consonance, while *Pear* (**23**, pp. 56–88) considers fusion, analyzability, pleasantness and unpleasantness, and association the factors which may enter into the perception of consonance.

Another purely psychological theory is that of *Münsterberg* (15, p. 88) who believes that fusion is strictly a mental experience. He, too, stresses the danger of confusing pleasantness and unpleasantness with degree of fusion.

Perhaps the most important of the genetic theories is that of *Ogden* (21, pp. 140–149) who advocates the harmonic theory of fusion, making consonance a synthesis based on association.

Myers (19, p. 316) also supports the theory of natural basis of association, but he assumes individual rather than racial experience as a possible source of fusion.

Valentine (36, pp. 118–135) likewise adheres to the "habit" theory. He believes that the phenomenon of adaptation with practice to discords lends strong support to the theory that the perception of consonance and dissonance is dependent upon frequent association.

Moore (14, pp. 68ff) has attempted to provide this genetic theory of consonance with an experimental basis in records of individual practice with dissonant intervals.

Review of experimental studies. Experimental work in the study of musical intervals began with *Helmholtz* (8), who, in 1863, constructed a curve of consonance using as a basis the number of beats contained in each interval. The significance of combinational tones had already been stressed by *Tartini* (32). By excluding overtones and combinational tones in his experiments, *Preyer* (26) found further that the affective judgment of agreeableness and disagreeableness seemed to depend on these excluded overtones and difference tones.

Fusion was used as a basis for the study of intervals in the experimental work of *Stumpf* (33) and *Faist* (2). The laws of fusion developed by Stumpf were subsequently verified by Faist. The measurement of consonance by means of reaction time was attempted by *Max Meyer* (16) and *Stumpf* (33), but this method was found to be unreliable, showing no consistency in the ranking of intervals. Two rankings of consonance are presented by *Buch* (1). Fusion is used as the basis in one and smoothness in the other.

The method of "paired comparisons" in ranking intervals was originated by *Meinong* and *Witasek* (**13**, pp. 189–205), who used violin tones and recognized fusion as the sole criterion for consonance. Following the same method, but using the Appunn Tonmesser, *Pear* (**23**, pp. 56–88) likewise ranked the intervals on the basis of fusion. "Purity" was the criterion used by *Krueger* (**9**, pp. 294–330) in his experiments with tuning forks. These and other experiments have been summarized in a table by *Malmberg* (**12**, p. 103) showing the order of consonances and dissonances.

Recent experiments in the ranking of intervals stress even more emphatically the complex nature of the phenomena of consonance. While *Myers* (**20**, pp. 92–94) placed no specific emphasis on the cognitive judgment, nevertheless his experimental work showed that the attitude of the subject was of prime importance in the control of the experiment.

The aesthetic attitude was encouraged by *Valentine* (**36**, pp. 118–135), all judgments being based on "preference" rather than "degree of fusion." Valentine found striking changes in the appreciation of discords, some or all of which became pleasing when repeated. He concludes that order of preference is very different from that of degree of consonance.

Likewise, *Max Meyer* (**17**, p. 207), in his experiments with quartertone music composed after the style of some Asiatic music, found that most of the *O*s considered the music displeasing at first but pleasing after several repetitions.

The method of paired comparisons was also used by *Malmberg* (**12**, pp. 93–133) in his experimental study of consonance. He attributes the failure to reach an agreement in the ranking of consonance and dissonance to a disagreement as to what constitutes consonance. "The perception of consonance is a cognitive process, involving the factors of blending, smoothness, and purity" (**12**, p. 131). An attempt was made by *Gaw* (**4**, pp. 134–147) to simplify the definition and concept of consonance. She found that, apparently, the greatest source of error in the Seashore consonance test was the failure to adhere

strictly to the definition of consonance, by allowing musical agreeableness to play an important rôle in the decisions.

In his functional study of consonance, *Peterson* (25, pp. 17–33) criticizes the various theories for making consonance more simple than it has actually been found to be. Concerning the judgment of consonance, he writes: "Consonance preferences are considerably influenced, we find, by numerous overlapping attitudes and sets engendered by the general relationships of tones under which the intervals reacted to are presented." *Peterson* (25, p. 33) stresses the importance of habituation in the experience of consonance.

An experimental study of the Seashore consonance test has been recently published by *Heinlein* (7, pp. 408–433). He criticizes the test for its unreliability, and draws the conclusion that paired interval comparison is an inadequate method for testing consonance, since "such comparison is conducive to reaction to the various elements of musical progression, including the feelings, which, apparently, are inseparably attached thereto" (7, p. 432). Furthermore, *Heinlein* (7, p. 433) assumes that "from the very nature and structure of the test material, there is reason to expect negative results from the talented group."

Outline of problem. In view of the conflicting opinions concerning the nature of consonance, and in view of the almost universal agreement that consonance is a complex mental phenomenon, our aim in this experiment was to discover how uniform the results of the Seashore consonance test are when the conditions are controlled as rigorously as possible and when the instructions are literally followed. Realizing the possibility of misconstruing instructions, *Seashore* (30, p. 14) has appended a footnote to the directions in the manual of instructions which accompanies the records: "This calls for a judgment on blending, smoothness, and fusion, apart from the feeling of like and dislike, and apart from theory or feeling of musical value. Blending, smoothness, and fusion should be explained fully and may be illustrated on the piano before the preliminary

practice." [2] Even when the instructions are faithfully carried out, lapses may occur in a practiced *O*.[3] Hence, it is extremely important that the directions be followed literally. Every effort was therefore made to counteract the tendency to judge consonance on the basis of pleasantness and unpleasantness.

As we have seen, the order of preference may be very different from that of the degree of consonance. The *O* does not necessarily prefer the more consonant of two intervals. In some combinations quite the reverse may be true. For example, in comparing the octave with the perfect fifth, the fifth would naturally be preferred, as it is a richer combination although less consonant than the octave. Furthermore, the experiments of *Meyer* (17, p. 21), *Moore* (14, p. 141), and *Valentine* (36, p. 141), which have been previously quoted, show that it is possible to grow used to dissonances and to accept as consonant intervals which do not readily fuse. Hence, "preference" must be eliminated as a criterion for consonance.[4]

Aside from a divergence of opinion concerning the inherent nature of consonance, there is almost general agreement that it is complex. The analysis is not as simple as that of pitch discrimination or of judgment of relative intensity of tones; of the discrimination of hues, tints, and saturations of colors; or of the analysis of tastes. Consonance, being allied to perceptual processes, involves imaginal as well as sensory factors: not only

[2] Heinlein (7, p. 41) admits that in his experimental work with the Seashore consonance test no explanation followed the reading of the instructions and that no preliminary practice was afforded. Consequently, affective rather than cognitive judgments very likely resulted.

[3] Peterson (25, p. 31): "Even under objectively controlled conditions various incidental, irrelevant stimuli received during the test and attitudes carried over from previous moments are bound to have their effects, thus altering the constancy of a subject's responses to any given pair of intervals."

[4] Apparently, this phase was neglected by Heinlein. All introspections quoted by him are affectively colored. One *O* reported that he "liked the smaller intervals better because they did not seem stretched out so far." Obviously this *O* was not attempting to make his judgment on "blending, smoothness, and fusion, apart from the feeling of like or dislike." Heinlein (7, p. 412) falls into a similar error in analyzing the test: "When the subject recorded that the second interval was 'worse' than the first interval, he thereby implied that he 'preferred' the first interval. Preference for the second interval was directly indicated by a judgment of 'better'."

are the sensory elements to be noted, but the imaginal factors that hark back to the individual's previous experience and to his contacts with the traditional standards of the race must be considered.[5]

This brings us directly to the type of judgment involved in the matter of consonant and dissonant intervals. It appears at once that since there are both sensory elements and imaginal background involved, the judgment calling for a fusion or "oneness" of the sensory blend may be influenced by traditional associations of an aesthetic nature. More specifically, too, the sounds produced will be colored also by affective processes. When one tone is declared to be higher, longer, or louder than another, as in some of the other Seashore tests for musical talent, there is scarcely any affective value attached to the experience. Certainly one note is not liked better than another because it is higher, louder, or longer, or the opposite of these in attributes.[6]

Experimental procedure. We proceeded then to investigate the consonance test under the conditions which are laid down in the manual of instructions as a safeguard to the test. The Seashore consonance test was given to a group of 132 students in an elementary psychology class at the University of Iowa. This group (Group A) was composed of students selected on the basis of scholarship under the Iowa plan of supervised instruction. The test was given on October 26, 1925, at 9 A.M. A Victrola, set for seventy-eight revolutions per minute, and loud steel needles were used for the test. Blanks were distributed for the recording of judgments. Before the record was played, the instructions were carefully read. Explanation and preliminary drill followed the reading of the instructions, and the *O*s were directed to make their judgments on the basis of blending, smoothness, and fusion, as far as possible. At the end of the

[5] Thus Stumpf was led to abandon the simple "act" explanation and to make allowance also for associative factors—a stand very definitely taken by Ogden, Myers, Valentine, Moore, and others.

[6] In his recent study of the problem Heinlein (7, pp. 408–433) has specifically called attention to these systematic differences but has done little or nothing to control factors that are obviously recognized in the instructions.

test the papers were collected and new blanks were distributed. After a brief intermission the instructions were repeated, and the consonance test was given again. Another record was used, and the *O*s were expected to infer that the second test was different from the first. This was done in order to discourage attempts to recall judgments made in the first test.

Six months later the same test was given for the third time to this same group of students at the same time of day as before. The same phonograph and records were used, and the same method of procedure was followed throughout. In order to ascertain the degree of musical training and experience of the *O*s, questionnaires similar to the one used by *Seashore* (**29**, p. 41), but with additional items (p. 78), were distributed. Tests 1 and 2 were compared and analyzed; later, tests 1 and 3 were similarly analyzed in terms of the following:

(1) Total number of errors for each interval
(2) Relation between particular intervals which were misjudged most commonly
(3) Different types of error (reversals, inconstancy of judgments, *etc.*)
(4) Consistent errors in repetitions of the same interval
(5) Comparison of first test and repetition in number and type of error [7]

As stated before, these *O*s represented a selected group. Lest conditions be too favorable in a class of this kind, one hundred fifty students in the unselected class in elementary psychology (Group B) were tested in similar manner. The first two tests were given February 11, 1926, at 9 A.M. and the third test was given one month later at the same hour. As before, the first and second tests and the first and third tests were compared and the results were expressed in tabular form.

On the basis of data secured from the questionnaire (p. 78) the records of thirty-five musically trained subjects (Group C) were analyzed and compared with those of thirty-five untrained

[7] This procedure corresponds in general to that of Heinlein (7, pp. 411–412).

subjects (Group D) in order to ascertain the effect of training or lack of training on the score).

In order to afford still better controlled conditions that could be more easily secured in the psychological laboratory, thirty-five musically trained *Os* (Group E), including piano, violin, and voice instructors, were tested individually. The same procedure was followed as in the group tests, except that the tests were given twice instead of three times.[8] Thirty-five untrained *Os* (Group F) were also tested individually under similar conditions. These records were then compared and the results tabulated as in the preceding experiments.

Discussion of results. A. *Gross comparisons.* 1. *Large groups, relatively unselected.* Comparing the results obtained in the first performance of the consonance test with those of the repetition several months later, we found a correlation coefficient of .63 with a P.E. of .022 (p. 79) for Group A of 150 cases (Table I), and .65, P.E. .024 for Group B of 132 cases (Table II). This was computed by the Pearson-product-moment method of correlation (27, p. 219).[9] These results compare favorably with consonance correlations obtained by *Peterson* (25, p. 32), who, in testing students, found a reliability coefficient of .68, P.E. .04, for 89 whites, and .52, P.E. .03, for 197 negroes. In a later study, *Peterson* (24, p. 36) obtained

[8] After testing several individuals three times (twice in succession, once at a later date) we discovered so little difference in the results that they were negligible. We therefore considered two tests sufficient for our present purposes.

[9] The reliability coefficient may be raised either by lengthening the test or by repeating the test and its duplicate twice each, averaging the two series of scores, and correlating these averages. A measure of this effect is obtained by applying Brown's "prophecy" formula:

$$r_x = \frac{Nr}{1 + (N-1)r} \qquad (3, \text{pp. } 269\text{–}270)$$

Substituting $r = .63$ and $N = 2$ in the formula, and solving for r_x we have:

$$(\text{Group A}) \quad r_x = \frac{2 \times .63}{1 + .63} = .77 \quad (\text{p. } 79)$$

$$(\text{Group B}) \quad r_x = \frac{2 \times .65}{1 + .65} = .79 \quad (\text{p. } 79)$$

a reliability coefficient of .59 for 130 white students and .41 for 170 negro students. In a group of 39 music students, *Gaw* (**6**, p. 8) found a correlation of .57, P.E. .06, while in a similar group of 42 cases the correlation was .486, P.E. .08. In both of these groups the consonance correlations were higher than those for the Seashore time test, which were .56, P.E. .06, and .458, P.E. .07, respectively. Results for the pitch and intensity tests were considerably higher than those for consonance.

In a group of 200 university students, *Weaver* (**38**, p. 170) found a mean of 69.17 and a standard deviation of 4.43. The average mean for our group of 132 cases (Group A) was 67.41 and the standard deviation 4.09 (p. 79). For Group B of 150 cases the mean was 66.695 and the standard deviation 4.095 p. 79).

Tables I and II show a high correlation between the results of Test 1 compared with Test 2, and Test 1 compared with Test 3. Thus, immediate repetition produces approximately the same results as the repetition several months later. The relatively insignificant difference in the means and reliability coefficients of Group A (Table I) selected on the basis of scholarship, and the unselected group (Group B, Table II), seems to corroborate studies establishing a zero correlation between general intelligence and ability to judge consonance.

2. *Small groups selected on the basis of musical training.* For the 35 musically trained *O*s, Group C (Table III), selected on the basis of data in questionnaires, we found a mean of 75.88, while for the 35 untrained *O*s, Group D (Table IV), selected in similar manner, the mean was 63.49. The mean of the 35 trained *O*s, Group E (Table V), who were tested individually, was 78.75, while that for the corresponding untrained group, Group F (Table VI), was 60.79. It is interesting to note that the means of the trained *O*s in the group test is almost equivalent to that of trained *O*s tested individually. There is also little variation in the mean of the untrained *O*s, whether tested in groups or singly. Relatively few significant changes in individual scores are noticeable and there is no general tendency to

score either higher or lower on the repetition of the test. The mean of the trained *O*s is considerably higher than that of the untrained *O*s both in individual and group tests. The comparative scores and the means of the musically trained and untrained *O*s are shown in Table VIII. Only two *O*s in the trained Group C (Table III) and one *O* in trained Group E (Table V) fell below the mean of the corresponding untrained groups. Similar results were found by *Gaw* (4) in a survey of musical talent in the school of music of Northwestern University. Only one *O* was found to be below the average in the sense of consonance. *Stanton* (29) found corresponding results in the Eastman School of Music.

The trained *O*s vary from the untrained *O*s in the type of error as well as in the number of errors. More constancy of errors is found in the judgments of the former. For instance, in pairs numbered 25 and 41 we find fifteen trained *O*s (Group E) who err consistently in both tests, choosing the minor sixth rather than the major sixth even when the intervals are reversed. A greater number of incorrect judgments are found among the untrained *O*s (Group F) but only four records show consistency of error. Similarly, in pairs numbered 23 and 43, 19 of the 27 errors recorded by musically trained *O*s (Group E) are constant, while none of the 32 errors in the judgments of the untrained (Group F) are consistent. The relative constancy of error is shown in Table VIII. Besides greater individual constancy of error in the talented group, we also find a greater general uniformity of judgment among trained *O*s than among the untrained or the unselected groups. In Tables III and V we find the errors centered about particular types of judgment, whereas in Tables I, II, IV, and VI greater diversity of error is shown.

The intervals showing the highest and lowest scores, with relative frequency of error in the various groups, are shown in Table IX.

B. *Analysis of intervals.* Among the musically trained *O*s cognitive judgments are apparent even in cases in which affective elements would be most apt to enter. For instance, in pair num-

ber 28 (Table V) only three *O*s in the first test and two *O*s in the second test considered the major third more consonant than the octave.[10] Ninety per cent of the *O*s judged correctly in both tests. If these *O*s had judged on the basis of preference, it is very likely that the major third would have been chosen as a richer and more aesthetically pleasing combination than the octave. Furthermore, if the factor of melodic direction were significant it would very probably operate here as pair number 28 represents a marked descent to a relatively rich combination.[11]

Neither do our results show a predominating influence of harmonic progression. If this factor were as prevalent as *Heinlein* (5, p. 416) indicates, we should expect judgments favoring the major third rather than the perfect fourth in pair number 23. Here we have the subdominant tonic relation. In passing from the former to the latter, an impression of completeness is supposedly conveyed. A study of the results in Table V shows that twenty of the musically talented *O*s (Group E) consistently judged the perfect fourth to be more consonant than the major third in pairs numbered 23 and 43. Most authorities substantiate this judgment, although Seashore recognizes the major third as more consonant than the perfect fourth.

Again, if harmonic relationship were used as the basis for decision, in pair number 38 we should expect *O*s to choose the perfect fifth rather than the octave, as this pair of intervals represents a passage from the dominant to the tonic, through the melodic skip in the lower tones. On the contrary, only three *O*s in the first test (Group E, Table V) and one *O* in the second test considered the perfect fifth more consonant. Ninety per cent. of the *O*s judged correctly in both tests. This same pair of intervals scored 63 per cent. in Group A, 63 per cent. in Group B, 80 per cent. in Group C, 56 per cent in Group D, and 51 per cent. in Group F (Tables I, II, III, IV, and VI, respectively.)

[10] Pair numbers 12 (ab–aa') and 28 (aa'–ac') do not appear in the tables, as they are not reversed. Through an error, pair number 28 on Seashore's record (aa'–ac' ♯) does not conform with the notation for that pair (aa'–af') in the Seashore text (26, p. 15).

[11] Heinlein (5, p. 416) states that in descending from a relatively high tone to a relatively low tone the low tone is almost invariably preferred.

Nevertheless, it might be argued that the element of anticipation might have influenced our experimental results. In our investigation the introspections of the *O*s reveal the fact that the Aufgabe, or mental set, is an exceedingly important factor which controls the *O*'s reaction to the test. One *O* states: "The idea of progression rarely, if ever, appeared, since there was in general a half and half probability that the interval would be resolved or would not be resolved; *i.e.,* it might get better or worse. There is no resemblance between this test and any form of musical melody and the feeling of finality is cancelled by the knowledge that the test guarantees no such assurance." Another *O,* with eight years of musical training, who made a score of 88 in the first test and 90 in the second, writes: "I took this same test two years ago and made a score of 46, but my judgments at that time were made solely on the basis of pleasantness and unpleasantness. I feel certain that I made a higher score this time, due to a better understanding of the instructions." However, as stated before, under the best of conditions lapses in attention may occur even among trained *O*s. This probably accounts for judgments favoring the minor second and major second rather than the octave in pairs numbered 13 and 14, respectively. Furthermore, fluctuation of attention may be responsible for occasional affective judgments. One *O* states: "Affective influences at times seemed to alter judgments made with relatively low concentration." Another *O* writes: "Affective bias was noticeably present only in combinations which were exceedingly unpleasant." This brings us to the study of the harshest dissonances. In pairs numbered 14 and 27, sixteen *O*s (Table VI) in the first test and fourteen *O*s in the second considered the minor seventh more consonant in the first pair and the minor second in the second pair. Eleven *O*s made these judgments in both tests. A preponderance of similar errors were found in pairs 19 and 47 (major seventh and minor second). Proportionally more errors of this nature were found in the untrained and unselected groups (Tables I, II, IV, and VI). Again, we find general disagreement among authorities concern-

ing the relative ranking of these particular intervals, some reversing the order as given by Seashore and others ranking all four intervals (major and minor second and major and minor seventh) equivalently. *Helmholtz* (8, pp. 186–187) gives the major seventh and minor second equal rank as the harshest dissonances.[12]

Conclusions. 1. In the light of correlations which we have found, and in the light of analysis of intervals in the records of trained, untrained, and unselected *O*s, we find that the procedure of paired-interval comparison is adequate for testing consonance. We have not found that the principle of harmonic progression was a sufficiently disturbing factor to distort our scores.

2. The Seashore consonance test, as verified by the method of retesting, is reliable either as a group test or as an individual test, provided that the instructions are literally followed.

3. Affective judgments may be eliminated to a great extent, under properly controlled conditions.

4. The relatively high scores of the *O*s with musical ability seem to indicate that the musically talented tend to possess an outstanding ability to judge consonance. On the other hand, it is understood that a high score in the Seashore consonance test is not necessarily indicative of musical capacity. This test is simply one of a series of tests which may be used effectively for measuring musical talent.

Critical comments. In view of the fact that this test involves more complex judgments than some of the other Seashore tests, it might be well to incorporate the footnote in the instructions for the test (thus guarding against the possibility of its being overlooked or slighted). It contains directions which are of vital significance to the understanding and proper administration of the test. Furthermore, in order to counteract the tendency

[12] There is also a variance of opinion among authorities regarding the ranking of the minor third and the major sixth. In pairs numbered 4 and 37 fifteen *O*s (Table V) in the first test and fourteen in the second favored the minor third rather than the major sixth. Ten *O*s chose the minor third consistently in both tests. In the other groups (Tables I, II, III, IV, and VI) similar reversals predominate, although the untrained *O*s (Tables IV and VI) show less consistency in their decision, choosing the major sixth in pair number four and the minor third in pair number 37, or *vice versa.*

to make judgments on the basis of preference, it might be safer to use other terms than "better" and "worse" to denote the degree of consonance. Too much affective significance is usually attached to these words. The method of recording "L" to signify less consonance and "M" more (for the second member of the pair in comparison with the first) was recently used effectively in a class in the psychology of music at the University of Iowa.

It might be well to note here that in one instance at least the correct answers to the test are at variance with opinions recorded by many expert investigators. Reference has already been made to the preponderance of opinion in favor of the perfect fourth as a better fusion than the major third. In the production of the Seashore consonance test, eight *O*s (**12**, p. 109), selected on the basis of their training and ability, discussed and criticized each pair of intervals until a unanimous verdict was reached. Unless this jury was an exception of juries in general, the criticism might be made that a few individuals could easily influence the votes of the rest. In any case, it is remarkable that the decisions of these judges, most of whom were not experts in this field of investigation, should be weighed against the statement of such authorities as Helmholtz, Stumpf, Krueger, Lipps, and others, who spent the major portion of their lives in this field of research.

The criticism which has been directed by *Heinlein* (**7**, p. 419) and others against the method of grading and percentile ranking of the consonance test is somewhat justified. No discrimination is made between different types of error. Although the pairs of intervals present varying degrees of difficulty, no more credit is allowed for the correct judgment of the most difficult than for that of the simplest pair. For instance, judging a minor second to be more consonant than an octave is considered as no worse an error than reversing the ranking order of a major third and a perfect fourth, or an octave and a perfect fifth. This seems especially significant in the higher scores. To illustrate, a score of 76 signifies a percentile rank of 86. An additional error of any kind reduces the percentile rank to 78, while two errors reduce

it to 68. Thus, an *O* who consistently reverses the ranking of such intervals as the major third and perfect fourth, or other pairs concerning which even experts disagree, may be penalized 18 per cent. for that particular pair of intervals. The raw score, therefore, appears to be more significant of the *O*'s ability, and for this reason it was used exclusively in our experimental work.

Explanation of tables. The data obtained in this study are compiled in Tables I to IX, inclusive. Table I shows the distribution of judgments in the three tests given to Group A, composed of 132 subjects selected on the basis of scholarship. Table II gives the results obtained in the unselected group of 150 students (Group B). Table III represents the records of Group C, 35 musically trained *O*s, selected on the basis of information derived from the questionnaires (p. 78). The records of 35 untrained *O*s (Group D), selected in similar manner, are shown in Table IV. Tables V and VI show results obtained in individual tests given to 35 trained and 35 untrained *O*s, respectively. The comparative scores of the musically trained and the untrained *O*s, both in group and individual tests, are shown in Table VII. Table VIII shows the relative consistency of error in the trained and the untrained *O*s. These results were obtained from a comparison of Group E (Table V) and Group F (Table VI). The intervals scoring highest and lowest with the relative scores in all groups are shown in Table IX.

TABLE I (Group A)

Intervals by Pairs		1 1 Test			2 2 Test			2 1 Test			1 2 Test		
		a	b	both	a	b	both	a	b	both	a	b	both
1 — 40													
Maj. 3*	Maj. 7	14	7	9	4	11	2	3	—	—	14	17	86
Maj. 7	Maj. 3*	16	9	7	6	9	—	2	4	1	7	9	93
2 — 39													
Aug. 4*	Maj. 7	32	21	6	18	22	9	11	11	—	12	19	44
Maj. 7	Aug. 4*	27	24	11	21	26	6	7	11	4	20	14	36
6 — 35													
Min. 7*	Maj. 2	14	13	22	7	3	1	9	18	16	22	18	41
Maj. 2	Min. 7*	21	11	15	4	5	4	14	16	11	15	22	48
8 — 33													
Maj. 3*	Min. 6	21	16	9	11	13	—	14	18	—	20	19	57
Min. 6	Maj. 3*	27	29	3	8	17	3	9	14	5	38	22	39

TABLE I (Group A)—*Continued*

Intervals by Pairs		1		1	2		2	2		1	1		2
		Test			Test			Test			Test		
		a	b	both	a	b	both	a	b	both	a	b	both
9 — 32													
Min. 7*	Maj. 2	36	24	11	11	14	6	12	16	—	20	25	36
Maj. 2	Min. 7*	41	32	6	14	18	3	12	12	—	12	17	44
11 — 30													
Min. 3*	Maj. 2	11	5	1	18	7	20	18	2	3	—	33	61
Maj. 2	Min. 3*	4	5	8	12	12	26	12	5	9	6	12	55
12 — 29													
Maj. 6*	Maj. 7	10	10	1	9	5	6	2	11	—	25	20	79
Maj. 7	Maj. 6*	7	2	4	3	7	12	1	4	1	18	16	86
14 — 27													
Min. 7*	Min. 2	42	34	11	12	10	—	17	19	—	13	21	37
Min. 2	Min. 7*	45	27	8	8	19	4	9	13	8	21	24	29
15 — 26													
Perf. 4*	Min. 2	6	—	2	2	—	1	—	—	—	—	8	121
Min. 2	Perf. 4*	4	2	4	3	2	—	—	3	—	2	2	119
19 — 47													
Maj. 7*	Min. 2	21	22	17	4	3	—	6	3	—	19	22	65
Min. 2	Maj. 7*	18	10	20	2	4	2	4	7	2	23	25	61
20 — 46													
Min. 3*	Dim. 5	20	9	7	10	15	11	9	6	2	15	24	58
Dim. 5	Min. 3*	14	9	13	13	10	8	5	6	6	11	28	62
22 — 44													
Maj. 6*	Min. 7	31	19	10	16	16	8	17	21	4	13	21	33
Min. 7	Maj. 6*	27	22	14	19	18	5	12	6	9	—	12	46
24 — 42													
Oct. *	Maj. 2	5	3	4	4	3	—	2	2	—	6	9	111
Maj. 2	Oct. *	2	—	7	1	2	3	1	—	1	3	5	114
		1		1	2		2	1		2	2		1
3 — 38													
Perf. 5	Oct. *	6	6	4	10	4	6	10	8	3	7	15	86
Oct. *	Perf. 5	8	4	2	7	2	9	8	4	5	—	13	93
4 — 37													
Min. 3	Min. 6*	10	7	—	16	19	7	16	21	9	23	18	51
Min. 6*	Min. 3	7	15	3	18	18	5	22	18	3	16	12	58
5 — 36													
Dim. 5	Min. 6*	14	16	8	9	12	6	11	10	—	16	12	68
Min. 6*	Dim. 5	12	14	10	12	17	3	7	11	4	24	13	60
7 — 34													
Maj. 3	Maj. 6*	18	16	7	22	27	6	19	22	4	19	13	37
Maj. 6*	Maj. 3	15	19	10	25	18	3	16	15	7	8	12	48
10 — 31													
Min. 2	Maj. 2*	22	12	10	16	14	5	11	11	—	6	18	62
Maj. 2*	Min. 2	26	15	6	14	9	7	9	15	2	—	10	68
16 — 50													
Dim. 5	Maj. 3*	10	7	14	6	9	12	14	7	6	13	20	57
Maj. 3*	Dim. 5	15	12	9	4	6	14	8	9	12	19	19	51

TABLE I (Group A)—*Continued*

Intervals by Pairs		*1* 1 Test a	b	both	2 *2* Test a	b	both	*1* *2* Test a	b	both	2 1 Test a	b	both
17	— 49												
Maj. 2	Min. 6*	2	3	1	13	20	7	6	4	4	21	15	78
Min. 6*	Maj. 2	—	4	3	11	7	9	8	5	2	12	15	87
18	— 48												
Min. 7	Perf. 4*	7	12	—	14	11	7	5	9	—	25	19	74
Perf. 4*	Min. 7	4	6	3	11	5	10	2	—	3	10	16	89
21	— 45												
Dim. 5	Min. 6*	16	13	14	12	10	2	8	7	2	18	24	60
Min. 6*	Dim. 5	10	11	20	9	5	5	10	4	—	10	19	68
23	— 43												
Perf. 4	Maj. 3*	8	8	1	24	35	33	11	23	5	35	12	15
Maj. 3*	Perf. 4	9	4	—	29	39	28	8	18	8	28	13	22
25	— 41												
Min. 6	Maj. 6*	26	19	11	14	14	—	21	19	9	10	19	41
Maj. 6*	Min. 6	30	27	7	9	15	5	24	26	6	19	14	32

The number shows which one of the two intervals was chosen while italics indicate an error in judgment; *e.g.*, 1 *1* signifies that the first interval was chosen in both pairs, the first judgment being correct, and the second incorrect.

a = test 1; b = test 2 or 3
1st line of each pair = comparison of tests 1 & 2
2nd line of each pair = comparison of tests 1 & 3

* indicates greater consonance according to Seashore manual

TABLE II (Group B)

Intervals by Pairs		1 *1* Test a	b	both	*2* 2 Test a	b	both	*2* *1* Test a	b	both	1 2 Test a	b	both
1	— 40												
Maj. 3*	Maj. 7	12	9	16	4	8	7	7	9	—	8	5	96
Maj. 7	Maj. 3*	15	15	13	8	11	3	5	10	2	19	11	85
2	— 39												
Aug. 4*	Maj. 7	10	18	9	12	4	15	9	7	4	15	17	76
Maj. 7	Aug. 4*	7	13	12	16	8	11	7	4	6	8	13	83
6	— 35												
Min. 7*	Maj. 2	28	31	13	26	29	—	14	17	—	23	14	46
Maj. 2	Min. 7*	32	34	9	22	26	4	7	14	7	36	23	33
8	— 33												
Maj. 3*	Min. 6	31	33	9	26	24	—	18	21	—	24	21	42
Min. 6	Maj. 3*	27	23	13	21	26	5	14	14	4	12	11	54
9	— 32												
Min. 7*	Maj. 2	14	9	11	9	12	14	5	—	8	26	33	63
Maj. 2	Min. 7*	21	11	4	13	16	10	4	6	9	19	27	70
11	— 30												
Min. 3*	Maj. 2	11	7	9	12	6	10	8	8	12	14	24	74
Maj. 2	Min. 3*	8	14	12	15	18	7	13	4	7	22	22	66

TABLE II (Group B)—*Continued*

Intervals by Pairs		1	*1*		*2*	2		*2*	*1*		1	2	
		Test			Test			Test			Test		
		a	b	both	a	b	both	a	b	both	a	b	both
12 — 29													
Maj. 6*	Maj. 7	7	12	7	11	16	5	7	—	4	21	18	88
Maj. 7	Maj. 6*	4	18	10	13	12	3	4	3	7	29	17	80
14 — 27													
Min. 7*	Min. 2	23	21	27	12	18	14	13	12	4	15	12	42
Min. 2	Min. 7*	29	18	21	18	22	8	11	14	6	24	28	33
15 — 26													
Perf. 4*	Min. 2	11	5	—	6	9	4	3	—	—	9	15	117
Min. 2	Perf. 4*	6	9	5	7	11	3	2	6	1	15	4	111
19 — 47													
Maj. 7*	Min. 2	14	21	6	15	9	7	6	11	4	23	17	75
Min. 2	Maj. 7*	7	16	13	10	4	12	3	15	7	29	14	69
20 — 46													
Min. 3*	Dim. 5	13	22	9	14	7	9	11	4	4	19	24	71
Dim. 5	Min. 3*	12	24	10	17	11	6	7	6	8	29	24	61
22 — 44													
Maj. 6*	Min. 7	41	27	13	12	21	7	14	11	—	17	25	46
Min. 7	Maj. 6*	36	29	18	14	11	5	11	16	3	5	10	58
24 — 42													
Oct. *	Maj. 2	9	4	—	6	8	3	5	—	—	12	20	115
Maj. 2	Oct. *	6	5	3	7	6	2	3	5	2	6	6	121

		1	1		2	*2*		*1*	*2*		2	1	
3 — 38													
Perf. 5	Oct. *	4	3	—	12	18	11	16	7	—	5	9	102
Oct. *	Perf. 5	2	—	2	18	24	5	11	11	5	13	9	94
4 — 37													
Min. 3	Min. 6*	12	16	3	21	18	4	19	24	6	21	15	64
Min. 6*	Min. 3	10	10	5	17	22	8	15	16	10	29	23	56
5 — 36													
Dim. 5	Min. 6*	21	19	9	16	26	4	12	—	—	16	20	72
Min. 6*	Dim. 5	17	15	13	14	23	6	9	8	3	24	18	64
7 — 34													
Maj. 3	Maj. 6*	22	26	9	24	22	5	21	20	4	17	16	48
Maj. 6*	Maj. 3	15	17	16	17	14	12	16	11	9	8	14	57
10 — 31													
Min. 2	Maj. 2*	31	21	9	16	24	7	14	19	—	15	12	58
Maj. 2*	Min. 2	29	23	11	18	27	5	11	11	3	25	22	48
16 — 50													
Dim. 5	Maj. 3*	18	12	10	17	21	14	19	12	—	16	25	56
Maj. 3*	Dim. 5	21	24	7	13	16	18	15	12	4	28	25	44
17 — 49													
Maj. 2	Min. 6*	9	12	—	24	31	12	11	6	—	18	13	76
Min. 6*	Maj. 2	7	16	2	29	28	7	9	14	2	30	17	64
18 — 48													
Min. 7	Perf. 4*	15	18	7	18	18	6	5	12	—	24	14	75
Perf. 4*	Min. 7	17	12	5	14	11	10	5	9	—	15	19	84

TABLE II (Group B)—*Continued*

Intervals by Pairs		1 1 Test			2 2 Test			1 2 Test			2 1 Test		
		a	b	both	a	b	both	a	b	both	a	b	both
21 — 45													
Dim. 5	Min. 6*	12	9	15	18	14	6	3	11	6	29	28	61
Min. 6*	Dim. 5	14	10	13	11	9	13	9	16	—	23	22	67
23 — 43													
Perf. 4	Maj. 3*	11	8	12	22	29	34	7	10	12	25	18	27
Maj. 3*	Perf. 4	18	4	5	27	23	29	6	18	13	21	27	31
25 — 41													
Min. 6	Maj. 6*	31	37	12	15	12	—	23	21	9	28	27	32
Maj. 6*	Min. 6	27	26	16	11	19	4	19	9	13	21	24	39

Legend: See Table I.

TABLE III (Group C)

Intervals by Pairs		1 1 Test			2 2 Test			2 1 Test			1 2 Test		
		a	b	both	a	b	both	a	b	both	a	b	both
1 — 40													
Maj. 3*	Maj. 7	1	2	—	—	1	—	3	1	3	3	3	25
Maj. 7	Maj. 3*	1	1	2	—	1	—	2	—	2	—	1	28
2 — 39													
Aug. 4*	Maj. 7	2	4	—	—	—	2	4	—	3	1	3	26
Maj. 7	Aug. 4*	1	4	1	1	2	1	2	—	2	5	3	22
6 — 35													
Min. 7*	Maj. 2	3	3	1	2	2	—	5	6	10	3	2	11
Maj. 2	Min. 7*	2	3	2	—	3	2	7	3	8	6	6	8
8 — 33													
Maj. 3*	Min. 6	3	3	2	2	—	3	3	2	—	2	5	21
Min. 6	Maj. 3*	4	4	1	1	2	3	2	1	1	4	4	19
9 — 32													
Min. 7*	Maj. 7	3	2	—	1	1	—	4	2	—	2	5	25
Maj. 7	Min. 7*	2	1	1	1	2	—	2	3	2	5	4	22
11 — 30													
Min. 3*	Maj. 2	2	1	1	2	2	—	—	—	4	2	3	24
Maj. 2	Min. 3*	1	—	2	1	—	1	—	—	4	—	1	26
12 — 29													
Maj. 6*	Maj. 7	2	2	—	1	1	—	2	1	4	—	1	26
Maj. 7	Maj. 6*	1	3	1	1	—	—	2	—	4	1	2	25
14 — 27													
Min. 7*	Min. 2	—	1	3	1	—	2	3	3	10	5	5	11
Min. 2	Min. 7*	—	—	3	—	1	3	5	4	8	2	2	14
15 — 26													
Perf. 4*	Min. 2	1	1	—	—	—	—	1	—	—	2	2	31
Min. 2	Perf. 4*	1	—	—	—	2	—	—	—	1	3	2	30
19 — 47													
Maj. 7*	Min. 2	2	2	—	3	—	—	4	4	4	5	8	17
Min. 2	Maj. 7*	1	3	1	3	1	—	5	3	3	3	5	19

TABLE III (Group C)—*Continued*

Intervals by Pairs		1	1		2	2		2	1		1	2	
		Test			Test			Test			Test		
		a	b	both	a	b	both	a	b	both	a	b	both
20 — 46													
Min. 3*	Dim. 5	2	2	—	2	3	—	2	3	6	3	1	20
Dim. 5	Min. 3*	1	—	1	—	4	2	3	—	5	—	—	23
22 — 44													
Maj. 6*	Min. 7	2	3	2	—	—	1	3	2	5	2	2	20
Min. 7	Maj. 6*	1	3	3	—	1	1	2	—	6	1	—	21
24 — 42													
Oct. *	Maj. 2	2	2	—	2	1	—	—	—	—	3	4	28
Maj. 2	Oct. *	1	1	1	2	2	—	—	1	—	1	—	30
			1	1		2	2		1	2		2	1
3 — 38													
Perf. 5	Oct. *	1	—	2	—	—	1	—	—	3	1	2	27
Oct. *	Perf. 5	—	—	3	—	—	1	1	—	2	—	1	28
4 — 37													
Min. 3	Maj. 6*	4	—	—	2	2	4	3	5	5	4	6	13
Maj. 6*	Min. 3	2	—	2	2	—	—	2	4	5	7	9	15
5 — 36													
Dim. 5	Min. 6*	—	—	—	2	—	2	3	3	8	—	2	20
Min. 6*	Dim. 5	—	1	—	—	—	4	2	3	9	2	—	18
7 — 34													
Maj. 3	Maj. 6*	—	2	2	3	2	4	6	3	7	4	6	9
Maj. 6*	Maj. 3	—	3	2	4	2	3	8	4	5	2	5	11
10 — 31													
Min. 2	Maj. 2*	2	4	—	2	—	—	1	—	1	1	3	27
Maj. 2*	Min. 2	1	3	1	3	—	1	—	—	2	3	3	25
16 — 50													
Dim. 5	Maj. 3*	1	3	—	2	1	2	1	3	1	6	3	22
Maj. 3*	Dim. 5	1	2	—	—	—	4	—	3	2	10	6	18
17 — 49													
Maj. 2	Min. 6*	3	—	2	2	—	—	—	—	4	—	5	24
Min. 6*	Maj. 2	2	—	3	—	—	2	1	1	3	—	2	24
18 — 48													
Min. 7	Perf. 4*	2	—	—	3	—	—	1	1	—	1	6	28
Perf. 4*	Min. 7	1	—	—	2	1	1	—	—	1	2	4	27
21 — 45													
Dim. 5	Min. 6*	1	4	4	2	—	3	2	2	—	8	7	15
Min. 6*	Dim. 5	2	2	2	1	2	4	1	—	2	4	4	19
23 — 43													
Perf. 4	Maj. 3*	—	—	2	3	2	2	2	4	10	4	3	12
Maj. 3*	Perf. 4	—	1	2	2	—	3	—	4	12	6	3	10
25 — 41													
Min. 6	Maj. 6*	4	3	—	—	—	2	4	4	9	3	4	13
Maj. 6*	Min. 6	2	—	2	—	—	2	2	6	11	6	4	10

Legend: See Table I.

TABLE IV (Group D)

Intervals by Pairs		1 1 Test a	b	both	2 2 Test a	b	both	2 1 Test a	b	both	1 2 Test a	b	both
1 — 40													
Maj. 3*	Maj. 7	2	5	2	3	—	—	2	2	2	6	6	18
Maj. 7	Maj. 3*	4	4	—	2	2	1	4	4	—	4	4	20
2 — 39													
Aug. 4*	Maj. 7	5	6	2	4	4	3	6	3	2	4	6	9
Maj. 7	Aug. 4*	3	4	4	4	3	3	8	4	—	—	4	13
6 — 35													
Min. 7*	Maj. 2	5	6	5	4	4	4	6	3	—	2	4	9
Maj. 2	Min. 7*	6	6	4	6	2	2	4	—	2	4	12	7
8 — 33													
Maj. 3*	Min. 6	5	4	2	6	7	—	6	6	—	7	7	9
Min. 6	Maj. 3*	4	6	3	5	6	1	4	7	2	10	4	6
9 — 32													
Min. 7*	Maj. 7	6	5	2	3	4	1	4	4	2	10	7	7
Maj. 7	Min. 7*	4	7	4	3	4	1	3	—	3	7	6	10
11 — 30													
Min. 3*	Maj. 2	2	—	2	3	3	2	4	2	2	—	4	20
Maj. 2	Min. 3*	1	1	3	4	4	4	4	—	2	—	4	17
12 — 29													
Maj. 6*	Maj. 7	2	4	2	6	4	2	3	3	3	2	2	15
Maj. 7	Maj. 6*	1	2	3	4	3	4	4	—	2	—	4	17
14 — 27													
Min. 7*	Min. 2	6	5	4	7	7	—	6	8	—	8	7	4
Min. 2	Min. 7*	5	3	5	4	6	3	4	4	2	4	4	8
15 — 26													
Perf. 4*	Min. 2	—	—	1	2	—	4	2	4	4	3	7	15
Min. 2	Perf. 4*	1	—	—	5	4	1	4	3	2	7	6	11
19 — 47													
Maj. 7*	Min. 2	4	4	3	2	2	—	2	4	4	8	6	12
Min. 2	Maj. 7*	2	3	5	1	—	1	6	5	—	4	5	16
20 — 46													
Min. 3*	Dim. 5	4	4	4	3	3	2	3	4	2	8	7	9
Dim. 5	Min. 3*	2	2	6	4	2	1	2	5	3	3	2	14
22 — 44													
Maj. 6*	Min. 7	4	6	4	2	5	—	6	6	4	6	1	9
Min. 7	Maj. 6*	3	4	5	1	4	1	5	—	5	3	4	12
24 — 42													
Oct. *	Maj. 2	2	4	2	3	—	2	2	3	—	4	4	20
Maj. 2	Oct. *	3	2	1	1	1	4	1	2	1	6	6	18
		1 1			2 2			1 2			2 1		
3 — 38													
Perf. 5	Oct. *	4	2	3	2	4	2	4	2	2	4	6	14
Oct. *	Perf. 5	2	2	5	—	—	4	3	1	3	2	4	16
4 — 37													
Min. 3	Min. 6*	4	4	—	5	6	5	3	4	3	6	4	9
Min. 6*	Min. 3	2	5	2	8	8	2	6	6	—	9	6	6

TABLE IV (Group D)—*Continued*

Intervals by	Pairs	1 Test a	1 Test b	both	2 Test a	2 Test b	both	1 Test a	2 Test b	both	2 Test a	1 Test b	both
5	— 36												
Dim. 5	Min. 6*	3	4	3	8	6	2	4	—	—	4	9	11
Min. 6*	Dim. 5	4	4	2	5	5	5	3	—	1	6	9	9
7	— 34												
Maj. 3	Maj. 6*	4	3	2	6	4	2	4	4	—	5	8	12
Maj. 6*	Maj. 3	4	2	2	4	5	4	4	3	—	7	9	10
10	— 31												
Min. 2	Maj. 2*	5	4	4	2	3	—	—	—	—	6	6	18
Maj. 2*	Min. 2	4	2	5	1	2	1	—	1	—	3	3	21
16	— 50												
Dim. 5	Maj. 3*	6	6	5	4	2	2	4	—	1	1	7	12
Maj. 3*	Dim. 5	4	4	7	3	4	3	5	—	—	—	4	13
17	— 49												
Maj. 2	Min. 6*	4	—	2	6	8	—	2	2	—	5	7	16
Min. 6*	Maj. 2	2	2	4	5	6	1	2	—	—	8	9	13
18	— 48												
Min. 7	Perf. 4*	3	1	—	2	4	3	3	—	—	4	7	16
Perf. 4*	Min. 7	2	2	1	3	4	2	2	—	1	4	5	13
21	— 45												
Dim. 5	Min. 6*	4	4	3	4	2	2	3	1	—	5	9	14
Min. 6*	Dim. 5	3	6	4	4	—	2	2	2	1	7	8	12
23	— 43												
Perf. 4	Maj. 3*	5	4	2	7	7	9	4	5	—	2	2	6
Maj. 3*	Perf. 4	3	3	4	6	6	10	3	2	1	—	1	8
25	— 41												
Min. 6	Maj. 6*	7	6	2	6	7	2	8	6	2	3	5	5
Maj. 6*	Min. 6	6	2	3	6	4	2	6	6	4	—	6	8

Legend: See Table I.

TABLE V (Group E)

Intervals by	Pairs	1 Test 1st	1 Test 2nd	both	2 Test 1st	2 Test 2nd	both	2 Test 1st	1 Test 2nd	both	1 Test 1st	2 Test 2nd	both
1	— 40												
Maj. 3* Maj. 7	Maj. 7 Maj. 3*	1	—	—	—	—	—	—	—	—	—	1	34
2	— 39												
Aug. 4* Maj. 7	Maj. 7 Aug. 4*	2	1	2	1	—	—	1	—	—	1	4	28
6	— 35												
Min. 7* Maj. 2	Maj. 2 Min. 7*	4	3	4	3	1	—	4	2	9	2	7	9
8	— 33												
Maj. 3* Min. 6	Min. 6 Maj. 3*	1	6	1	4	2	1	5	3	6	4	3	13

Table V (Group E)—*Continued*

Intervals by Pairs	1	1		2	2		2	1		1	2	
	Test			Test			Test			Test		
	1st	2nd	both	1st	2nd	both	1st	2nd	both	1st	2nd	both
9 — 32 Min. 7* Maj. 7 Maj. 7 Min. 7*	—	—	—	1	—	—	—	—	5	—	1	29
11 — 30 Min. 3* Maj. 2 Maj. 2 Min. 3*	—	—	1	3	—	3	—	—	2	—	3	26
12 — 29 Maj. 6* Maj. 7 Maj. 7 Maj. 6*	1	1	—	—	2	—	—	—	6	1	—	27
14 — 27 Min. 7* Min. 2 Min. 2 Min. 7*	—	—	1	—	—	—	5	3	11	5	7	13
15 — 26 Perf. 4* Min. 2 Min. 2 Perf. 4*	2	1	1	—	1	—	1	—	—	1	2	30
19 — 47 Maj. 7* Min. 2 Min. 2 Maj. 7*	2	—	1	1	—	—	4	5	6	4	6	17
20 — 46 Min. 3* Dim. 5 Dim. 5 Min. 3*	2	1	—	3	1	—	1	—	2	2	6	25
22 — 44 Maj. 6* Min. 7 Min. 7 Maj. 6*	3	—	3	—	2	—	—	—	3	2	3	24
24 — 42 Oct. * Maj. 2 Maj. 2 Oct. *	—	—	—	—	1	—	—	—	—	1	—	34

Intervals by Pairs	1	1		2	2		1	2		2	1	
3 — 38 Perf. 5 Oct. * Oct. * Perf. 5	—	—	—	—	1	—	—	—	3	1	—	31
4 — 37 Min. 3 Maj. 6* Maj. 6* Min. 3	1	2	—	1	2	4	5	4	10	4	3	10
5 — 36 Dim. 5 Min. 6* Min. 6* Dim. 5	1	—	—	2	—	—	1	3	5	2	3	24
7 — 34 Maj. 3 Maj. 6* Maj. 6* Maj. 3	3	1	—	2	1	4	4	5	6	4	6	12
10 — 31 Min. 2 Maj. 2* Maj. 2* Min. 2	1	—	—	1	1	—	2	2	2	1	2	28
16 — 50 Dim. 5 Maj. 3* Maj. 3* Dim. 5	4	2	5	2	1	—	2	—	—	6	11	16

Table V (Group E)—*Continued*

Intervals by Pairs	1	1		2	2		1	2		2	1	
	Test			Test			Test			Test		
	1st	2nd	both	1st	2nd	both	1st	2nd	both	1st	2nd	both
17 — 49 Maj. 2 Min. 6* Min. 6* Maj. 2	2	—	—	—	—	1	—	—	—	—	2	32
18 — 48 Min. 7 Perf. 4* Perf. 4* Min. 7	1	1	2	3	1	3	2	—	—	2	6	22
21 — 45 Dim. 5 Min. 6* Min. 6* Dim. 5	5	2	—	1	3	—	1	1	—	5	6	23
23 — 43 Perf. 4 Maj. 3* Maj. 3* Perf. 4	—	—	—	3	2	2	2	1	19	1	3	8
25 — 41 Min. 6 Maj. 6* Maj. 6* Min. 6	1	1	—	2	2	4	5	3	11	2	4	10

Legend: See Table I.

Table VI (Group F)

Intervals by Pairs	1	1		2	2		2	1		1	2	
	Test			Test			Test			Test		
	1st	2nd	both	1st	2nd	both	1st	2nd	both	1st	2nd	both
1 — 40 Maj. 3* Maj. 7 Maj. 7 Maj. 3*	6	6	2	1	5	2	4	—	—	2	2	18
2 — 39 Aug. 4* Maj. 7 Maj. 7 Aug. 4*	4	3	3	—	3	—	4	5	—	8	5	16
6 — 35 Min. 7* Maj. 2 Maj. 2 Min. 7*	9	7	—	6	9	—	7	5	2	7	8	4
8 — 33 Maj. 3* Min. 6 Min. 6 Maj. 3*	6	4	—	4	4	3	5	6	—	8	9	9
9 — 32 Min. 7* Maj. 7 Maj. 7 Min. 7*	3	2	—	6	4	5	4	5	—	5	7	12
11 — 30 Min. 3* Maj. 2 Maj. 2 Min. 3*	2	5	2	4	3	—	3	3	2	8	6	16
12 — 29 Maj. 6* Maj. 7 Maj. 7 Maj. 6*	4	4	4	6	3	—	2	5	2	2	2	16
14 — 27 Min. 7* Min. 2 Min. 2 Min. 7*	6	6	4	5	5	2	5	5	—	6	6	7

TABLE VI (Group F)—*Continued*

Intervals by Pairs		1	1		2	2		2	1		1	2	
		Test			Test			Test			Test		
		1st	2nd	both	1st	2nd	both	1st	2nd	both	1st	2nd	both
15	— 26												
Perf. 4*	Min. 2	3	—	—	7	9	—	4	3	1	4	6	16
Min. 2	Perf. 4*												
19	— 47												
Maj. 7*	Min. 2	8	6	1	8	8	—	—	4	—	7	5	12
Min. 2	Maj. 7*												
20	— 46												
Min. 3*	Dim. 5	7	5	1	6	6	—	4	7	—	7	6	10
Dim. 5	Min. 3*												
22	— 44												
Maj. 6*	Min. 7	6	4	—	3	5	2	6	6	—	6	6	12
Min. 7	Maj. 6*												
24	— 42												
Oct. *	Maj. 2	3	3	2	2	1	—	1	2	—	4	4	23
Maj. 2	Oct. *												
		1	1		2	1		2	2		1	2	
3	— 38												
Perf. 5	Oct. *	4	4	1	3	5	2	4	—	—	3	5	18
Oct. *	Perf. 5												
4	— 37												
Min. 3	Maj. 6*	6	8	—	9	7	4	4	4	3	3	3	6
Maj. 6*	Min. 3												
5	— 36												
Dim. 5	Min. 6*	6	8	2	3	4	—	4	4	—	8	5	12
Min. 6*	Dim. 5												
7	— 34												
Maj. 3	Maj. 6*	6	7	—	6	4	2	5	5	—	6	7	10
Maj. 6*	Maj. 3												
10	— 31												
Min. 2	Maj. 2*	4	4	2	6	3	—	4	4	—	2	5	17
Maj. 2*	Min. 2												
16	— 50												
Dim. 5	Maj. 3*	8	7	3	5	4	—	2	—	—	5	9	12
Maj. 3*	Dim. 5												
17	— 49												
Maj. 2	Min. 6*	6	4	4	3	2	—	2	—	—	1	6	19
Min. 6*	Maj. 2												
18	— 48												
Min. 7	Perf. 4*	5	3	2	4	5	—	1	3	—	5	4	18
Perf. 4*	Min. 7												
21	— 45												
Dim. 5	Min. 6*	3	4	2	6	5	—	4	5	—	5	4	15
Min. 6*	Dim. 5												
23	— 43												
Perf. 4	Maj. 3*	6	3	2	13	13	2	5	5	—	4	7	3
Maj. 3*	Perf. 4												
25	— 41												
Min. 6	Maj. 6*	9	8	4	7	9	—	6	4	—	3	4	6
Maj. 6*	Min. 6												

Legend: See Table I.

TABLE VII. *Per cent of correct judgments for first and second trials of the Consonance Test*

	Trained Os				Untrained Os			
	Group Tests		Individual Tests		Group Tests		Individual Tests	
	I	II	I	II	I	II	I	II
	78	72	88	92	66	66	66	56
	70	60	86	88	66	58	76	80
	76	76	70	60	66	58	64	68
	84	78	80	86	54	50	58	58
	82	86	86	84	62	54	66	68
	80	82	54	64	54	46	70	64
	76	84	88	90	62	48	50	46
	72	76	76	68	68	74	78	60
	74	76	80	78	68	48	58	58
	74	72	76	70	62	54	48	60
	68	68	88	86	72	70	68	50
	78	76	70	82	60	60	72	66
	74	72	82	78	52	56	50	60
	76	82	76	72	74	72	52	58
	82	86	80	82	68	66	56	66
	78	76	86	80	60	66	64	58
	82	78	90	84	78	80	62	58
	74	80	86	76	68	70	54	50
	80	78	78	84	56	56	66	62
	76	74	76	76	46	56	74	66
	76	70	78	74	78	76	46	48
	86	82	76	72	74	76	66	50
	80	86	80	78	60	58	58	66
	82	78	76	74	60	52	60	56
	74	74	80	78	78	76	70	78
	78	78	78	86	66	66	52	58
	80	86	82	84	54	56	70	60
	78	76	82	84	72	76	64	54
	54	60	80	86	66	68	60	52
	78	80	86	86	58	66	74	70
	68	64	78	84	64	62	64	64
	60	56	76	56	76	62	52	58
	72	78	80	76	70	72	46	54
	80	86	84	78	60	62	72	60
	78	76	68	54	52	58	60	56
Mean	75.89	75.88	79.49	78	64.28	62.68	61.88	59.71
"	75.885		78.745		63.48		60.795	
"	77.32				62.14			

TABLE VIII. *Consistency of judgments in trained and untrained groups* (In per cent of *O*s)

Intervals by Pairs		Trained *O*s a	Trained *O*s b	Trained *O*s c	Untrained *O*s a	Untrained *O*s b	Untrained *O*s c
1	40						
Maj. 3*	Maj. 7	97	—	97	51	11	62
Maj. 7	Maj. 3*						
2	39						
Aug. 4*	Maj. 7	80	06	86	45	—	45.
Maj. 7	Aug. 4*						
3	38						
Perf. 5	Oct. *	88	08	96	51	08	59
Oct. *	Perf. 5						
4	37						
Min. 3	Min. 6*	28	40	68	17	20	37
Min. 6*	Min. 3						
5	36						
Dim. 5	Min. 6*	68	14	82	34	05	39
Min. 6*	Dim. 5						
6	35						
Min. 7*	Maj. 2	25	56	81	11	06	17
Maj. 2	Min. 7*						
7	34						
Maj. 3	Maj. 6*	34	28	62	28	05	33
Maj. 6*	Maj. 3						
8	33						
Maj. 3*	Min. 6	37	23	60	25	08	33
Min. 6	Maj. 3*						
9	32						
Min. 7*	Maj. 7	83	14	97	34	14	48
Maj. 7	Min. 7*						
10	30						
Min. 2	Maj. 2*	80	06	86	48	05	53
Maj. 2*	Min. 2						
11	31						
Min. 3*	Maj. 2	74	17	91	46	05	51
Maj. 2	Min. 3*						
12	29						
Maj. 6*	Maj. 7	77	17	94	46	17	63
Maj. 7	Maj. 6*						
Average		64.25	19.08	83.33	36.33	8.66	44.99

a: Per cent of observers judging correctly in both tests.
b: Per cent of observers erring consistently in both tests.
c: Total per cent of observers judging consistently in both tests (a–b).

TABLE IX. *Intervals scoring highest and lowest*

(In per cent of *O*s in each group)

Highest		Group Table	A II	B III	C IV	D V	E VI	F VII	Average of all Groups
24 — 42									
Oct. * / Maj. 2	Maj. 2 / Oct. *		86	79	83	54	97	66	77.5
15 — 26									
Perf. 4* / Min. 2	Min. 2 / Perf. 4*		67	60	76	43	97	51	65.7
1 — 40									
Maj. 3* / Maj. 7	Maj. 7 / Maj. 3*		90	76	87	37	85	46	70.2
17 — 49									
Maj. 2 / Min. 6*	Min. 6* / Maj. 2		63	58	74	41	91	54	63.5
12 — 29									
Maj. 6* / Maj. 7	Maj. 7 / Maj. 6*		62	56	73	46	77	46	60
11 — 30									
Min. 3* / Maj. 2	Maj. 2 / Min. 3*		52	47	71	53	74	46	57.2
Lowest									
23 — 43									
Perf. 4 / Maj. 3*	Maj. 3* / Perf. 4		13	19	31	20	23	10	19.3
6 — 35									
Min. 7* / Maj. 2	Maj. 2 / Min. 7*		33	25	27	23	26	11	24.1
25 — 41									
Min. 6 / Maj. 6*	Maj. 6* / Min. 6		28	23	33	18	33	17	25.1
19 — 47									
Maj. 7* / Min. 2	Min. 2 / Maj. 7*		27	21	41	28	39	26	30.1
4 — 37									
Min. 3 / Maj. 6*	Maj. 6* / Min. 3		30	40	39	21	33	17	31.6
8 — 33									
Maj. 3* / Min. 6	Min. 6 / Maj. 3*		36	32	57	21	37	25	34.6

A: *O*s selected on the basis of scholarship.
B: Unselected *O*s.
C: Trained *O*s (Group tests).
D: Untrained *O*s (Group tests).
E: Trained *O*s (Individual tests).
F: Untrained *O*s (Individual tests).

QUESTIONNAIRE FOR SEASHORE CONSONANCE TEST

Name Academic Year Major Date

Please give as specific and detailed information as possible in regard to the following:

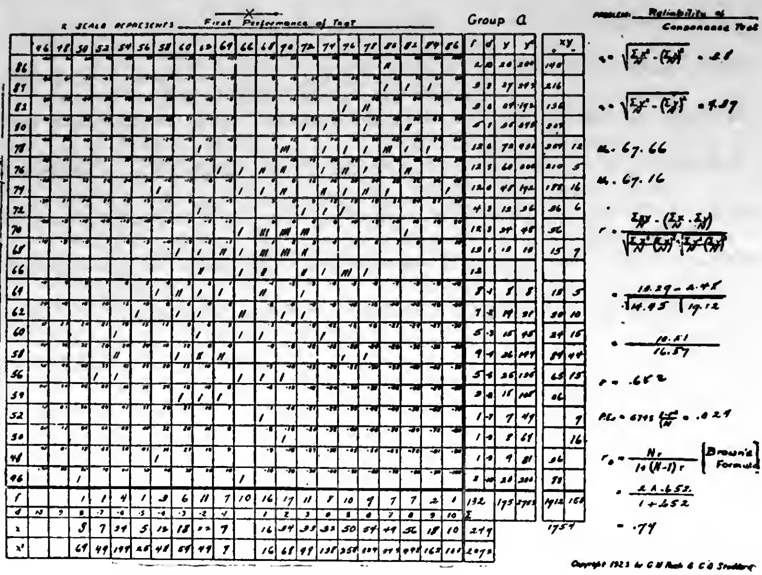
1. Musical Training
   1. In public schools
   2. Private vocal lessons (when, where, how long, *etc.*)
   3. Private instrumental lessons (when, where, how long, *etc.*)

II. Musical Environment
   1. Instruments in your home, and their use
   2. Opportunities for hearing music of any sort (specific)

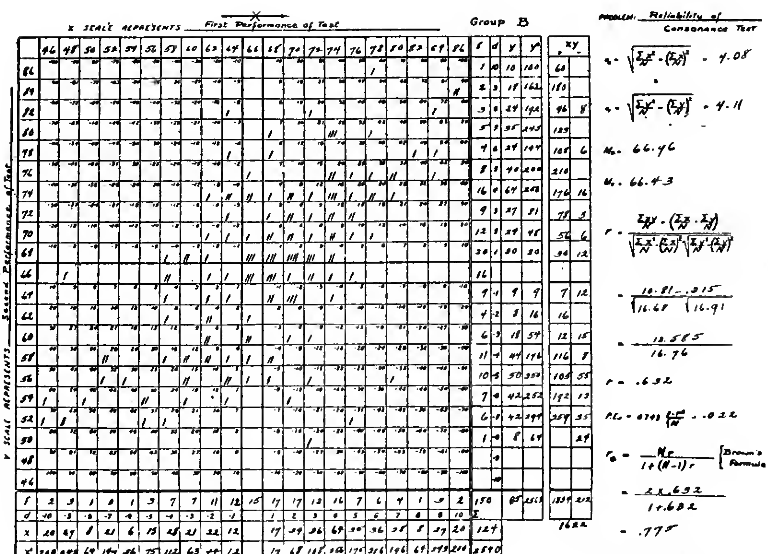
III. Musical Expression
   1. Favorite selections you can sing (by ear? by note?)
   2. Favorite selections you can play (by ear? by note? instruments?)
   3. Singing or playing in public (parts, occasions, *etc.*)

IV. Enjoyment of Music (what actually appeals to you)
   1. Vocal (solo, quartette, chorus, opera, popular songs, classics, religious, secular)
   2. Instrumental (solo, symphony, band, *etc.*)
   3. Characteristic effects of music (mental, physical)

V. Additional Data
   1. Have you had any courses in harmony?
   2. To what extent can you recognize intervals?
   3. As far as you know, did any elements of harmonic progressions enter into our judgments?
   4. To what extent did you allow your judgment to be influenced by pleasant and unpleasant effects?



Group A



Group B

Correlation between two performances of the Seashore Consonance Test

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THE MODE OF VIBRATION OF THE VOCAL CORDS [1]

by

WOLFGANG METZGER

Research Associate in the University of Iowa, 1926–27

I. Historical review: pre-laryngoscopic period; laryngoscopic period; earlier stroboscopic period; recent period; the new models and latest publications.
II. Experimental investigation: analysis of the main argument; the stroboscope; the action of different models; the shape of the vocal cords; the perceived tone; the cause of the double vibrations.
III. Summary; bibliography; figures and photographs.

The problem of this paper comprises the following subproblems: (1) What is the direction of the vibrations of the human vocal cords? Do they vibrate transversally, that is, in the direction of the air blast, or laterally—perpendicularly to the direction of the air blast, or in some other direction and curve? (2) What are the relations between the movements of the two cords? Do they move synchronously; in other words, are corresponding points of them under normal conditions always in symmetrical position or have their movements some other relation to each other? A solution of the above problems would also answer the question about the ratio between the frequency of the cords and the frequency of the produced tone; and also the technical question, which kind of functioning larynx model can be supposed adequately to reproduce the action of the real vocal cords. The point of attack of the present investigation is the voice of an adult male in the lower pitches or " chest " registers.

[1] This study was carried out in the psychological laboratory of the State University of Iowa under the guidance of Professor C. E. Seashore to whom the writer wishes to acknowledge his deep indebtedness; he is also greatly indebted to Professor G. W. Stewart of the department of physics, Professor Prentiss of the department of anatomy, Dr. Dvorak of the nose and throat clinic, Dr. M. Metfessel, Miss A. Mathiesen, and Mr. V. W. Allaben of the department of philosophy and psychology, Dr. S. T. Barrows of the department of speech, Mr. Yarnell of the hydromechanic laboratory of the department of engineering, and finally the instrument maker, Mr. Dempster, for their kind advice and assistance.

I. *Historical Review*

1. *Pre-laryngoscopic period.* The history of our problem is not identical with the history of the problem of the function of the larynx in general, which starts with *Hippokrates* about 430 B.C. It begins with the assumption that the central factor in voice production is the vibration of the vocal cords. According to *Musehold*,[2] the oldest known vibration theory of voice production is at the same time much closer to the truth than most of the following one. *Johann Jacob Heinrici* (1681) was the first to consider the periodic widening and narrowing of the vocal slit as the chief cause of the phonation. In his assumption the cavities of the pharynx and the mouth have the same function in producing and changing the voice as have the tubes of trumpets, trombones, flutes, and other wind instruments. Obviously in this theory the lateral component of the vibration of the vocal cords is the essential one. One might further suppose that the theory requires a symmetrical movement of the two cords, but, as we will see later, the question is not quite so simple.

In 1734 *Cyrillus*[3] compared the vocal cords with the strings of a violin and the air passing along with the violin bow. This idea was suggested by *Dodard's* discovery in 1700 that the pitch of the voice depends on the tension of the vocal cords. The same assumption was made by *Anton Ferrein* in 1741, who was the first to approach the problem experimentally. Ferrein made the first experiments on cadavers' larynxes. He used larynxes of dogs, pigs, and cows. He also tried to reproduce the functioning larynx with elastic membranes,[4] but at present the writer

[2] Musehold '13,* p. 93; extensive general surveys of the history of voice theories are given, besides Musehold '13, pp. 93–102, 111–117, by Liskovius in 1846, pp. 71–133, by Fournié in 1866, pp. 177–345, shorter ones by Grützner in 1879, pp. 72–79, 101–103, by Scripture in 1902, pp. 247–280, and by Wright '02, pp. 215, 216, 360. None of them can entirely replace the others because naturally each writer refers to studies written in his own language which are not mentioned by the others.

* [Ed. Numbers preceded by an apostrophe ('27) refer to the bibliography which is alphabetically arranged, the number itself referring to the year of publication.]

[3] Musehold, '13, p. 93, and Liskovius, p. 76.

[4] Scripture '02, p. 255, and Fournié, p. 279.

has no description of these devices at hand. Ferrein concludes, according to *Fournié,* " These bands, which in the future I shall call vocal strings, therefore can be compared with the double isochronous strings of the clavichord. The glottis or vocal slit is but the interval between them. The wind that hits the strings has the function of the plumes that pinch the strings of the clavichord." [5] At another place he compares the air blast with a violin bow. His assumption, therefore, is that the acoustical vibrations are transmitted to the air immediately from the surfaces of the vibrating cords. The width of the slit between them has only the function of allowing passage to the air blast. Ferrein could disprove the opinion of Aristotle, according to which the voice becomes louder when the slit is wider, by finding that widening of the slit diminished the strength of the tone and finally made it disappear. But rhythmical widening and narrowing of the slit as a possible cause of the tone has no place in his theory. His explanation explicitly stipulates merely that the two cords vibrate with the same frequency; and all we can safely assume to be implied by it is that the chief direction of the vibrations is transverse, that is, up and down in the larynx, otherwise they would not push the air with their surfaces as they are supposed to do. Symmetrical movement is not necessary to this theory; lateral movements are possible but of no importance;[6] the frequency of the produced tone is of course identical with the frequency of the cords.

The comparison of the mechanism of the vocal cords with the reed of a pipe occurs first with *Joseph Louis Roger* in 1758.[7] Shortly afterwards, in 1806, we find the comparison with the lips of a trumpet-blower stated again more explicitly by *Dutrochet.* He was led by his idea to the assumption that not only the edges but the whole thyro-arythenoid muscles vibrate. *Malgaigne,*[8] in

[5] All quotations from foreign languages are translated into English by the writer if a translation did not already exist.

[6] It must, however, be mentioned that in the aeolian harp which is the only known instrument of this kind the strings vibrate sidewise, not in the direction of the air blast, according to Scripture '02, p. 256.

[7] Musehold '13, p. 93, and Liskovius, p. 82.

[8] Fournié, pp. 305–311.

1831, took up the assumption of Dutrochet. He considered both the lips of a trumpet-blower and the thyro-arythenoid muscles as a kind of reed and added to the older comparison the point that the *ventriculi morgagni* above the vocal cords correspond to the hollow mouthpiece of the brass instruments. All these comparisons, however, suffer from the fact that at that time nothing was settled about the exact physics of reed pipes and the rôle of the blower's lips in the brass instruments.

Extensive investigations of the organs of voice production in animals were made by *Johannes Müller* and published chiefly in 1839 and 1840. He was the first to make extensive and systematic experiments with artificial models of the larynx besides his observations with cadavers' larynxes. By a remark of *Helmholtz* the erroneous impression was made that most of his model experiments were made with a plain tube, the flat end of which was covered by two rubber membranes which left a narrow slit between them (Fig. 1).[9] He chiefly studied the factors influencing the pitch, the intensity, and the "registers," and found such striking similarities in the mechanisms of his type of models and the cadaver's larynx that he considered the larynx as a pipe with two membranous reeds. He assumes that the exact mode of vibration is somewhat as follows: The vibrations of the vocal cords are transmitted to the air by their surfaces. The essential component of their movement, therefore, is transverse. Periodical changes in the width of the glottis are of no importance. Lateral components of the movement, therefore, may be disregarded. The relations between the movements of the two bands are of no relevance, except that a pure tone requires identical frequency of vibration. The air-vibration is supposed to have the same frequency as the bands. In other words, with regard to our question Müller's assumptions contain nothing more than an explicit statement of the theory of Ferrein. Instead of strings, membranes are assumed, but the laws are not changed.

During the next few years a great number of researches con-

[9] Helmholtz '12, p. 97. In Müller '40 only the flat pipe is treated, but in Müller '39 seven figures of complete membrane pipes are shown, six of which are oblique.

tinuing and supplementing Müller's studies were performed. *Lehfeldt,*[10] in 1835, and *Liskovius,* in 1846, furnished valuable information about the mechanism of the vocal cords at the different registers. *Harless*[11] replaced the rubber membranes of the model with frog muscles, and thus he could change the pitch by contraction as well as by extension. New features of the functioning larynx were hereby opened to experimental investigation. For our problem the following facts are of importance. Liskovius observed that during phonation the surface of the vocal cords is puffed up so that a drop of water put on the cords runs towards the walls and stays there. That means that the average position of the cords during phonation is not horizontal but somewhat roof-shaped. *Merkel's*[12] work contains 107 pages of investigation on double membranes, mostly lying in one plane, but also a few with an angle between them and with two opposite folds instead of two opposite edges. It is interesting to remark that he explicitly puts to himself the question whether in the flat model " during phonation the excursions and recursions of the two membranes occur at the same time or these processes occur alternately in such a manner that when the one band moves forward the other moves backward " (p. 411). His answer to this question is that the movements are always synchronous, and the fact that it sometimes looks the other way is considered by him as an optical illusion. But we must not forget that he had to rely on observations with the naked eye and that therefore a real solution of the question was not possible. Of equal interest are his doubts about the reliability of cadaver observations with regard to the functions of the living larynx:[13] " The tremendously important lack of muscle contractions in the dead larynx constitutes an enormous gap between the phenomena on either

[10] Lehfeldt discovered that in the chest registers the voice-lips vibrate in their whole width, whereas in the head registers only their edges vibrate. This was verified by Müller.

[11] Ewald '98 and Nagel '08 continued these experiments with contractile vibrators started by Harless (see also Scripture '02, '06, '20); other studies about similar subjects were made by Despiney and Kudelka in the middle of the last century, according to Wright.

[12] Merkel '57, pp. 402–409, 411, 509–560.

[13] Merkel '57, p. 519.

side. . . ." The real importance of these differences was recognized only much later, as we shall notice soon.[14]

2. *Laryngoscopic period.*[15] In all investigations up to the middle of the last century the basic assumption of Müller '40 is accepted as established; namely, that in reed pipes not the vibration of the air in the pipe but the vibration of the reed itself is primarily responsible for the perceived sound, in other words, that the reeds or membranes themselves are the sounding principle.[16] In fact, this assumption had already been disproved in 1825. *E. H.* and *W. Weber* showed that the reed in a pipe is "no self sounding body which transmits its sound to the neighboring air by striking it; for, if the reed is drawn away from its position of rest and then released, it gives only a very weak tone which cannot cause the air in the pipe to sound. It is rather a body which, by opening and closing the pipe alternately, forces the air compressed outside of the entrance of the pipe, to displace and not to displace the air inside the pipe in regular intervals ('25 § 287). Since a clear notion of this kind of action is indispensable for a full understanding of voice production, the later version of Helmholtz may be added:[17]

"The sound in these pipes is excited by intermittent pulses of air which at each swing break through the opening that is closed by the tongue of the reed. A freely vibrating tongue has far too small a surface to communicate any appreciable quantity of sonorous motion to the surrounding air; and it is as little able to excite the air enclosed in pipes. The sound seems really to be produced by pulses of air, as in the siren, where the metal plate that opens and closes the orifice does not vibrate at all. By the alternate opening and closing of a passage, a continuous influx of air is changed into a periodic motion, capable of affecting the air. . . . Now the motion of the air which passes through a siren, or past a vibrating tongue, is discontinuous in a very high degree, since the individual pulses of air must be generally separated by complete pauses during the closures of the opening. . . . A tone thus produced also shows that it is really due to puffs of air. I have examined the vibrating tongue of a reed pipe, . . . , when in action with the vibration

[14] See I, Section 4—especially the arguments of Ewald '98.

[15] The history of earlier laryngoscopy is found in Brown and Behnke, *Voice, Song, and Speech;* in Wright '02; and in Scripture '02, pp. 247–250; the recent development of monocular "endoscopy" before 1913 is described by Panconcelli '13 with a complete bibliography.

[16] Merkel '57, p. 345. It must be mentioned that Liskovius, p. 39, accepted the theory of E. H. and W. Weber without referring to Garcia in 1846.

[17] Helmholtz '12, p. 101.

microscope of Lissajous, in order to determine the vibrational form of the tongue, and I found that the tongue performed perfectly regular simple vibrations. Hence it would communicate to the air merely a simple tone and not a compound tone, if the sound were directly produced by its own vibrations."

These arguments are valid as well for membranous reeds as for ordinary reeds. Müller mentioned the experiments of Weber, but does not acknowledge their validity for the human voice organ.

This view changed only when the first successful inspection of the living vocal cords during their action was made by *Manuel Garcia.* His first observations date from the year 1841 but his theory of phonation was published in the form that is known now, only in 1855.[18] We quote from the *Philosophical Magazine:*

"But by virtue of what principle is the voice formed? It seems to me that the answer to this question can be but this; the voice is formed in one unique manner,—*by the compressions and expansions of the air, or the successive and regular explosions which it produces in passing through the glottis.* The ligaments of the glottis . . . close the passage, and present a resistance to the air. As soon as the air has accumulated sufficiently, it parts these folds and produces an explosion. But at the same instant,—by virtue of their elasticity, and the pressure from below being relieved, they meet again to give rise to a fresh explosion. A series of these compressions and expansions, or the explosions, occasioned by the expansive force of the air and the reaction of the glottis, produces the voice. This theory, though now generally admitted for reeds, and undoubtedly evident in the liquid vein, the toothed-wheel of Savart, the syrène of the Baron Cagnard Latour, etc., has not to my knowledge yet been applied to the glottis. (I find that Dr. Müller hints at the possibility of the voice being thus formed, but only to attack and reject the notion.) If we consider that the lips of this aperture, taken separately, can give no kind of sound, however we may try to make them speak, we must admit that the sounds which they give forth by their mutual operation are only due to the explosions of the air produced by their strokes. It is not necessary in order to obtain the explosion of sound, that the glottis should be perfectly closed each time after its opening; it suffices that it should oppose an obstacle to the air capable of developing its elasticity."

Merkel who repeated the observations of Garcia on his own larynx, offers the same interpretation concerning the kind of movement.[19] According to him the starting position of the edges of the vocal cords in the "chest register" is closed and straight; during each vibration the cords move upward and sideways and

[18] The quotation found in Scripture '02 is not taken from the original but is translated back into English by him from a German translation, probably by Muschold.

[19] Merkel '62 I, p. 633, and '62 II.

then back to the original straight position in which the glottis is closed for a moment. The falsetto and head registers differ only by the fact that the closure of the glottis is not complete there.

We shall now summarize the outcome of these studies for our problem. According to Garcia and Merkel, the direction of the vibrations is upward and outwards and back again so that in the lower position the glottis is closed or at least narrowest, and in the upper position it has its widest opening. The movement of the vocal cords, therefore, is exactly symmetrical or synchronous. Since each full vibration of the cords makes one opening of the glottis and thus allows one displacement of the air, the fundamental frequency of the produced tone is identical with the frequency of the cords.

In connection with these new ideas new larynx models were devised which gave a better representation of the opening and closing of the slit, for instance, the models of Helmholtz and Fournié.[20] Their common trait is that the flat membranes are definitely abandoned as possible representatives of the vocal cords.[21] Helmholtz's prescription is the following (Fig. 2a):

"Cut the end of a wooden or guttapercha tube obliquely on both sides, leaving two nearly rectangular points standing between the two edges which are cut obliquely. Then gently stretch strips of vulcanized India rubber over the two oblique edges, so as to leave a small slit between them, and fasten them with a thread. . . . When the membranes bend inwards the slit is closed, when outwards, it is open. Membranes which are fastened in this oblique manner speak much better than those which are laid at right angles to the axis of the tube, as Johannes Müller proposed, for in the latter case they require to be bent outwards by the air before they can begin to open and to shut alternately." [22]

[20] Helmholtz '12, p. 97, and Fournié, pp. 396–398.

[21] Cf. Footnote 9. Müller had also used oblique models besides the flat one that is known under his name, but in his theory ('40, pp. 174–179) there was no reason for preferring the oblique model. It is interesting that in the last sentence of our quotation Helmholtz carries the principle of action over from his model to the model of Müller; the inverse conclusion was made later by West.

[22] West had doubts about the meaning of the word "alternately" in Helmholtz's description; but Helmholtz's statement about the conditions of opening and closing of the slit shows clearly that, in his opinion, the position of the bands is always symmetrical, that the vibration is synchronous; and that the word "alternately" refers only to the alternation between openness and closeness of the slit.

The angle in Fournié's model (Fig. 2b) is especially acute; he simply takes the end of a light rubber tube at two opposite points and stretches the tube till the ends come together. The distinguishing feature of his model, however, is that he does not fasten the tube exactly at the end but somewhat below it so that the part above the dotted line in the figure is perfectly free. He believes that this is necessary for a perfect reproduction of the conditions of the larynx, for in his opinion the thyro-arythenoid muscles cannot themselves vibrate, as they are so stiff that to put them into a state of vibration would require such an enormous air pressure as the human breath never can produce. So he believes that in phonation, as well of the vocal cords as of the lips of a trumpet-blower, a part of the mucous membrane separates from the stiffened muscles, as is shown by Fig. 3, and thus forms the freely vibrating body. This assumed loose part of the mucous membrane is represented by the part above the dotted line in his model, whereas the dotted line itself in which the tension is greatest represents the stiff muscle. Strangely Fournié for some reason had not obtained any reasonable vibration with an arrangement similar to Helmholtz's, so he attributed much importance to the looseness of the proper edges.

The character of the vibration is clear in both models. The direction is mostly sidewise, with a slight upward component which is wholly unimportant in Fournié's model, a little greater in Helmholtz's. The position of the cords is always symmetrical; each full vibration of the edges gives one opening; thus, the frequency of the edges and the frequency of the perceived sound are the same. Other larynx models which were devised about the same time differ from the described ones only in the mechanisms of pitch control, not in the principle of action.

3. *Earlier stroboscopic period.* It is interesting that, in the last sentence of our quotation, Helmholtz carried over the principle of action from his oblique model to the flat model of Müller. As we shall soon see, this was not justified. But at this time there was no possibility of detecting the real differences

in the action of the two kinds of models. In fact, every trait of the membranous reeds that could possibly be detected with the naked eye and ear had been treated by the investigations of Merkel, Harless, *Grützner* and others. But nobody had yet seen a real vibration in process. For no optical events following each other in a time interval of less than one-fourth to one-twelfth of a second, according to the circumstances of observation, can be seen correctly because they give rise to all kinds of subjective phenomena—deviations of the seen movement from the objective, flickering stimulus which finally result in fusion. The slowest single vibration of the human vocal cords takes not more than one-eighty-first of a second, which is well below the valve for fusion. The observation of single phases which is indispensable for the exact knowledge of the vibration, therefore, requires a device which apparently slackens the movements of the vibrating body.

New progress was made possible when in 1876 *M. J. Oertel* introduced such a device into phonology, namely, the stroboscope.[23] Oertel started his study with the observation of a flat rubber model as it was used by Müller. The first thing he found was the strange fact that the two membranes did not move synchronously but alternately, one moving upward while the other moved downward. After this observation he systematically varied the tension of the membranes and found that the alternate vibration is caused by unequal tension of the two membranes and that it is replaced by synchronous movement if the tension of the membranes is exactly equal. His numerous observations on nodes are of minor interest for our problem. The chief result of his observations of the living larynx is that the vibrations of the vocal cords are transverse, that is, up and down, always synchronous, and without nodal lines across the vocal slit.

Oertel's observations on models were continued by *Koschla-*

[23] The chief names connected with the development of the stroboscopic method in laryngology are Oertel '95 who also gives some of the earlier history of this instrument since its invention by Stampfer at Vienna in 1832 and its first application to the observation of vibrations by Ernst Mach in 1873; further see L. Réthi '97 I and II, Nagel '08, Musehold '13, pp. 102–111, Hegener '14, Wethlo '15, Loebell, and Tonndorf '26.

koff in 1884[24] and 1886. For our investigation especially his second paper is of interest. It contains the first stroboscopic observations of oblique models. Because of their closer similarity to the larynx he used elliptical tubes cut off in four different angles: 180° (Müller), 160°, 130°, and 90° (Helmholtz). The chief results follow. Alternating vibration occurs most easily with the flat models, that is, with 180° or 160°. The membranes then represent double freely vibrating membranous reeds which make two slightly different openings of the slit at each single vibration. Synchronous vibration occurs most easily when the pipe is roof-shaped, that is, at the angles of 130° or 90°. Of his extensive observations on partial vibrations and on the effects of unequal tension a few may be mentioned. Nodes which divide the slit occur only when the middle is touched by something or when the edges touch each other in the middle. Koschlakoff also found a rather strong "mutual adaptation" of the two natural frequencies of the membranes when vibrating together; the common pitch, then, was not identical with either of the two natural frequencies. After the rubber models Koschlakoff observed through the stroboscope ten larynxes of cadavers which were cut off imediately above the vocal cords and found only synchronous vibration, partly total, partly nodal, but never was the slit divided by cross nodes. Further, he observed the larynxes of two normal professional singers, a baritone and a tenor. The vibrations were always synchronous.

Simanowski, who worked in the same laboratory with Koschlakoff in St. Petersburg, was the first to make photographic records from the larynx models. His paper of 1885 contains a number of very instructive figures with curves taken simultaneously from corresponding points of the two edges of Müller's flat model; first with equal tension and, therefore, synchronous vibration, then with unequal tension and, therefore, alternate vibration. But his paper of 1888 is of considerably greater interest for our present purpose. Its title is "The Vibration of the Vocal Cords

[24] In this study Koschlakoff investigated the possible causes of hoarseness and found interesting parallels in his model.

during Paralysis of Different Laryngeal Muscles." And its express purpose is to find out whether the alternate vibration which can be observed in artificial larynxes also occurs in the real vocal cords. Since in the artificial larynxes differences of tension had been found to be the cause of alternate vibration, he attempted to bring about such differences of tension in a number of dogs. In the first experiment the right crico-thyroid muscle was scratched out. When the dog had recovered his right vocal cord was found to lie below the left. During sounds of medium strength the vibration was now alternate; only during the strongest sounds was it still synchronous. Simanowski explains this latter phenomenon by the plausible assumption that at the greatest contraction the remained cricoid muscle acts also upon the other side and thus equalizes the tension of the two cords. This experiment was checked on several other dogs. In the second experiment a 7 cm. piece of the right upper laryngeal nerve was cut out on new dogs. Medium sounds then showed alternating vibration, and only the weakest sounds showed synchronous vibration, which is easily understood by the assumption that there is no innervation through the normal nerve and, therefore, no difference between the tensions of the vocal cord connected with the normal nerve and the vocal cord connected with the interrupted nerve. Later, in the same dog, synchronous vibration was also found during the loudest sounds which must be expected if the explanation of the same fact in the first experiment was right. This second experiment was performed with two dogs. In the third experiment the subjects of the first one were used. Its purpose was to reestablish the synchronous movement. This could be expected when both the right and the left cords were paralyzed. The upper left laryngeal nerve, therefore, was cut, as in Experiment 2, so that one vocal cord was paralyzed by the lack of a muscle, the other by the lack of a nerve. The result of this procedure was very slow synchronous vibration; alternate vibration, now, could not be obtained by any means. Simanowski then tried to find cases of one-sided paralysis of a human larynx, but he had only one case which

was not far from being cured. In this case the vibrations were synchronous but with smaller amplitude on the affected side.

In a paper published in 1896 Oertel verified the observation that in the normal larynx the vibrations are synchronous. He was also able to furnish more cases of diseased larynxes. One was a case of acute catarrh and, as in Simanowski's case, the amplitudes were unequal but the vibrations were synchronous. The other was a case of chronic catarrh. One cord was completely slack and moved alternately with the other cord, together with nodal fluttering. In two other cases of complete paralysis of one side the paralyzed cord did not move at all, and in a third case of the same kind alternating and synchronous movement followed each other. A reason for the changes could not be found.

This interesting phase of investigation was concluded by the work of *L. Réthi* in 1897.[25] He is chiefly concerned with the nodal lines that had been described by his predecessors; and concludes that there are no nodal vibrations of any kind in the normal human larynx. This was verified definitely by Musehold[26] in the same year. Réthi assumes synchronous vibration of the whole vocal cords in the chest register, of their edges only in the falsetto and head registers, as Lehfeldt had already assumed in 1835. According to Réthi the chief component of the vibration is transverse, that is, vertical in the throat, and the vocal cords open and close the glottal slit like folding doors.

What, then, are the common traits of the theories of the "earlier stroboscopic period" and their implications for our problem? Weber's theory of the reed pipes was generally accepted. The action of the larynx was no longer measured by movements and amplitudes of the cords directly, but by the changes in the width of the slit between the cords. These assumptions are more similar to Müller's than to Garcia's and Merkel's.[27] The primary movement of the vocal cords is assumedly not lateral but transverse. The pressure of the air

[25] Both papers.
[26] Musehold '97 and '13, pp. 114 ff.
[27] Not yet in Merkel '57.

loses the crucial importance that it had in Garcia's theory in producing the actual movements of the vocal cords. The determining fact, consequently, is a swinging operation in the manner of folding doors which at different moments allow passage to different amounts of air.

As we have seen, the assumption of an alternate movement, which was a possible assumption in this theory, was rejected empirically by a great number of observations. We may mention that *Nagel*[28] has doubts about the validity of these observations and thinks that nothing is settled until improved stereoscopic observation is applied. The observations on living subjects must be made through the throat mirror and can supposedly be made only monocularly. It is very hard, however, to see with one eye such small differences of distance exactly in the line of vision as must necessarily be observed in order to decide about the present problem. But it can be shown that this objection of Nagel is only valid for a perfect folding-door movement if the vibrations take place around a line connecting their axes, as the position of equilibrium. In this case, as Figs. 4 and 5 show, for an observer who is looking in the direction of the arrows position 4a does not differ from 4c, neither does 5a differ from 5c; supposed vertical (*viz.* transversal) differences are completely invisible; only symmetrical narrowing and widening of the slit can be seen; and, therefore, alternate vibration does not appear different from synchronous vibration, as Nagel supposes in his argument. But this is not true when the positions of rest of the two wings have an angle of less than 180°, which had already been found to be true for the human vocal cords by Liskovius. In this case, as can be seen in Figs. 6 and 7, no other than synchronous movements are represented to the observer by symmetrical widening and narrowing of the slit. When observed in the direction of the arrow, alternate movements (Fig. 6) necessarily carry with them an apparent alternate shifting of the slit from right to left and from left to right which can hardly escape the eye of a skilled observer and can hardly be confused with

[28] Nagel '08, p. 721 ff.

a plain widening and narrowing (Fig. 7), at least in the chest register where the moving bodies have sufficient size. Moreover, the experiments of Koschlakoff[29] with models showed that in the oblique position of the vibrators, as it was found in the human larynx by Liskovius, synchronous movement is more likely to occur than alternate movement. In spite of Nagel's objections we, therefore, can consider it as fairly well established by the aforementioned investigators that under normal conditions the two vocal cords vibrate synchronously, that is, that corresponding points of them are always in symmetrical position. The relations between the frequency of the cords and the frequency of the air wave, however, can not be stated so simply here as in former theories. If the vocal cords are considered as synchronously and freely vibrating reeds, they may have two moments of smallest opening during one vibration—one in swinging back, one in swinging forth—and, correspondingly, two points of greatest opening. These openings may either be equal, as in Fig 8a. The air-wave should then transmit the double frequency of the cords. The openings, on the other hand, may be different, as in Fig. 8b. The air-wave would in this case transmit the cord frequency of the octave. Only if the position of equilibrium of the vibrating cords forms such an acute angle that they could not swing back beyond the connecting line of the two axes (Fig. 8c) would the octave disappear. This would correspond to Garcia's assumption. Obviously, it would not make an essential difference whether or not the two edges hit each other in the narrowest position. The observations of Liskovius and Koschlakoff exclude the first of these three possibilities, but do not decide between the second and the third.

4. *Recent period.* About the same time as the publication of the described theories serious anatomical objections were made. Since the time of Johannes Müller the vocal cords had been treated more and more explicitly as membranes. Consequently at that time we find repeatedly the suggestion that the term "vocal cord" be replaced by "vocal band," "vocal ligament,"

[29] Koschlakoff '86.

or "vocal membrane." The first attack against this idea had already been made in 1877 by the physicist, *Carl Müller,* but seemingly was not known by Oertel and the others. The result of Carl Müller's findings is that the lower pitches of the chest register are produced by a mode of vibration foreign to the vibrations of membranes; that, in general, the "vocal lips," as he calls them, have not the least similarity with membranes in a physical sense since they consist of wedge-shaped muscular bodies covered by elastic tissue. He admits that in the higher pitches and especially in the falsetto the comparison with membranes has a somewhat better justification and experiments on rubber membranes, therefore, have some value since, by the increased tension, the shape of the vocal cords may be changed in such a manner that their wedge-shaped edge becomes sharper and sharper.

In 1893 *B. Fränkel* published his excellent microtomic frontal cuts of the vocal cords which made it perfectly clear that there is no similarity between the vocal cords and a membrane (Fig. 9). There are bundles of muscular fibers embedded in an elastic tissue which become more and more abundant towards the "edge" but nowhere give the slightest hint of a membrane-shaped process beyond the end of the muscular mass. The laryngeal cavity below the cords forms a very sharp wedge. Between the cords the walls are perfectly parallel for a short distance and, then, gently bend back into the *ventriculi morgagni.*

Indeed, Fränkel's cuts could not be made in the state of phonation, though by some ingenuity he endeavoured to reproduce it as closely as possible. On the other hand, there were the numerous experiments with larynxes of cadavers made by Johannes Müller, Harless, Koschlakoff, and many others, which seemed to agree perfectly with the experiments made on the models. But serious doubts were also brought forward against the validity of conclusions carried over from experiments on cadavers to the living larynx. The statement of *J. Richard Ewald* made in 1896 and published in 1898 in regard to this point may be summarized as follows: the vocal cords of the dead

larynx can only be made tense by lengthening, whereas in the living larynx the tension of the vocal cords can be produced by their own contraction without lengthening—even with some shortening. Every tension of the vocal cords of the cadaver's larynx, therefore, produced a wholly unnatural displacement of all its parts. That means that possibly the dead vocal cords under this unnatural lengthening have some resemblance to membranes, which is foreign to the living cords. Ewald concludes that the elementary physical conditions of the action of the larynx can more safely be studied with artificial models than with the complicated and no longer normally functioning anatomic specimen. Regarding the probable mode of vibration he argues thus: even assuming that in the living larynx during phonation the inferior walls of the cords are not so steep as in Fränkel's cuts, the fact remains that, by their contraction, at least in the chest register, they form thick rolls and not membranes. According to Weber and Helmholtz, the crucial factor in the action of the vocal cords is the change in the width of the glottal slit. If there were thin membranes moving in the direction of the air stream, by bending and thus changing their angle relatively slight displacements were sufficient for widening the slit considerably. But as there are thick bodies, movements of the same kind and of the same amount of displacement widen the slit much less. Only excessively large displacements in the direction of the air stream could produce such a widening as is observed through the throat mirror. It is much more probable that the vocal cords make this opening by yielding to one side, that is, that the cords are not bent but compressed.[30] In Figs. 10 and 11, Ewald gives two possible designs of pipes acting in the way described. In the first one the opposite bodies forming the slit are pressed together by their own elasticity, while, in the second, bodies themselves are rigid but are pressed against each other by elastic or other forces of their support. Ewald conjectures that in the human larynx perhaps both principles are combined; that is, that the pair of elastic bodies or cushions that

[30] Cf. the detailed arguments of Nagel '08 referred to in the last part of I, 4.

form the slit at the same time rests on an elastic support. He also admits the possibility that the larynx is neither a pure membraneous pipe nor a pure cushion pipe, but an intermediate form of both.

Since Ewald built his models and studied their action at a much later time, we will first consider the studies of Musehold and others which were published in the meantime. Without knowing Ewald's arguments, Musehold furnished the most valuable substantiation to them as early as 1897.[31] He took some photographs of the moving vocal cords through the throat mirror with explosive flash light and others with the stroboscope, and gave very careful descriptions of the cross-section of the vocal cords. According to his pictures the following description of their top aspect is correct. In the chest register the surface is lip-shaped, vaulted upward. The slit between them, which is more or less linear and not bordered by edges, forms the bottom of a groove having rounded walls which gradually become steeper toward the middle. When the same tone is sung a little softer the surface becomes slightly flatter, but the roll shape is still so prevalent that in the enlargement it can hardly be distinguished from mouth lips. Musehold correctly explains this shape and the mutual contact by the fact that in the chest register the chief means of tension is the contraction and, therefore, the thickening of the thyro-arythenoid of vocal muscle. Johannes Müller had already assumed that, but had not followed it through to Musehold's conclusions because he was so biased toward the idea of separate vocal " bands " that he treated the vocal muscle as something different and apart from them. From this interpretation it can be safely concluded that at least in the chest register the invisible inferior surface of the vocal cords is as rounded as the top surface, since it too must be bulged by the contraction of the vocal muscle; hence, the lower cavity of the larynx must be very narrow and like a sharp wedge in shape, as Fränkel had found in his cuts. In order to see the shape of this cavity when the vocal muscle is not contracted, Musehold brought

[31] All of the chief results of Musehold '97 are also contained in Musehold '13.

a number of larynxes of cadavers in the state of slight tension required for phonation. According to J. Müller's method, he turned them upside down and poured beeswax into the cavity. Thus he got the cross-sections shown in Fig. 12. This figure shows that even without any contraction of the vocal muscle, as in the higher registers, the lower surface of the vocal cords is never parallel to the upper surface, not even for the tiniest distance from the edge; but it is rounded and in its gross direction closer to the vertical than to the horizontal. This must be especially true when the vocal muscle is contracted. It means that under no condition have the vocal cords the shape of membranes. Musehold gives the following designs of the cross-section of the vocal cords in the chest register (Fig. 13). Under the conditions of this design no up and down vibration, and still less a swinging through in the downward direction, is possible. Only one general mode of vibration is possible, which had been assumed by Garcia; namely, " upward and outwards " at once either with or without complete closing. According to the direction of the chief component of the air pressure on the lower side walls, as shown in the figure, the sideward movement will be by far the stronger one. In other words, the enclosed air will act in the manner of a wedge on the lower walls of the vocal cords; the walls will, consequently be compressed and since the whole mass is soft the edges will move to the side and swell upward a little (see the figure). In attempting to get stroboscopic pictures of every phase of the vibration in the chest register, Musehold found that it was much harder to catch the phase of opening than the phase of closure. He concludes that the vocal cords do not only touch slightly but remain in the state of closure for a considerable period of time. The vibration curve hypothetically constructed by him bears close similarity to the curves found by Grützner in pipes with one striking reed (as in the oboe or saxophone). For the falsetto register Musehold's photographs show a narrower edge and no touching in the lower position, but free vibration. Here he admits, like Ewald, the possibility of an up and down movement; but the cross-sections which were derived

by him from the photographs, together with the beeswax casts of the inferior side, show that the lowest position always makes the slit narrowest. A second point of opening at the lower end of the vibration, as in Figs. 8a and 8b, is impossible. As to alternate vibrations in a cushion-pipe, there is no mechanical or, better, no geometrical objection to the assumption that one cushion yields toward the wall while the other proceeds toward the middle of the tube so that the extreme phases would look like Fig. 14. But there is a serious acoustical objection to this. As we know, the voice is produced by the rhythmical widening and narrowing of the air passage. But, as the figure shows, with alternate movement, instead of being widened and narrowed the slit would shift from one side to the other, back and forth, in a cushion-pipe, and the stream of air would constantly find practically the same opening. Besides, alternate vibrations of this prevailingly lateral direction when occurring in the human larynx would be less likely to escape stroboscopical inspection than under the conditions supposed to be true by Oertel, Koschlakoff, and Réthi. A glance at the excellent photographs of Musehold shows perfect symmetry throughout the whole series and, also, such extreme differences with regard to the width of the slit as could not occur with the described lateral, and at the same time alternate, mode of vibration. To summarize, Musehold supports by the most excellent stroboscopic photographs a theory in which the assumptions of Garcia and Merkel are reëstablished in full and which, therefore, need not be repeated.

According to their true shape the vocal cords henceforth were called *Stimmlippen,* that is, voice lips in German laryngology. It is of interest that Ferrein as well as Johannes Müller ('40, p. 197) were led to use this name by their anatomical studies of the organ and gave up this name later in behalf of their functional assumptions. As we know, the vocal cords had already been compared with the mouth lips of a trumpet-blower by Heinrici in 1681 and later by Dutrochet and by Malgaigne. Musehold could substantiate this comparison by stroboscopic observations of the lips of a man who blew into the mouthpiece of a large

trombone ('13, p. 130). He found that the movements were essentially sidewise and synchronous—in plain language, opening and closing the mouth. Moreover, he found there the differences of voice production known in the larynx as the two fundamental registers; namely, vibrations with complete closure of the lips which produce the sharp tones of the trombone, and vibrations without closure which produce the soft breathy tones of the trombone.[32]

Musehold's theory was accepted and supported with new arguments by *E. W. Scripture* in his 1901 paper on the nature of vowel sounds and his fundamental *Elements of Experimental Phonetics* (pp. 257–263). He says (p. 257) about the membrane pipes:

"They form convenient instruments for illustrating the effect of tension on the pitch of the membrane but are decidedly liable to mislead in implying that the vocal bands vibrate like membranes . . .", namely, that they are likely to vibrate transversely, that is, up and down in the throat. "The vocal bands suggest a pair of cushions suitable for compression, and not a pair of membranes" (p. 258). And finally (p. 263), "Studies of vowels sung and spoken in the chest register seem to require the assumption of cushion action of the bands, as indicated by the following facts. The movement imparted to the air by a freely vibrating membrane is necessarily of the nature of a sinusoid (p. 2) or a sum of harmonic sinusoids (p. 13). That the movement is not of such a nature in sung and spoken vowels of the chest register has been proven by recorded speech curves (p. 41). The vibrations of cushions may be of any degree of sharpness or smoothness from a practically instantaneous explosion to a movement as regular as that of a fork. Such vibrations appear in the various speech curves. Vibrations of a sinusoid character can arouse only harmonic resonance vibrations, whereas explosive vibrations require no such adjustment of the resonating cavity. The evidence is conclusive (pp. 21, 39) that in sung and spoken vowels a harmonic relation between the resonance tones and the cord tone is not necessary. In considering the tone aroused by the cords it seems necessary to treat it not as a note composed of a series of partials (p. 90) but as a series of puffs. These facts are conclusive in regard to the cushion action in the chest register. Similar data for the head register are not at hand."

Scripture's *Researches in Experimental Phonetics* of 1906 repeat this argumentation with a number of especially instructive designs from which all kinds of transverse movements, as, for instance, alternate up and down movements of the two cords, are

[32] These observations of the mouth lips were meanwhile checked by Panconcelli-Calzia ('27). The first part of his voice film, issued in the summer of 1927, shows the synchronous movement perfectly clearly.

obviously excluded. The result is that "the glottal lips open partly by yielding *sidewise*—that is, they are compressed—and partly by being shoved upward and outward." It is worth mentioning that in a small paper of 1920 Scripture does not find a reason for modifying these basic statements, but holds to them entirely.

In 1908 W. Nagel published the results of his own independent stroboscopic observations of the larynx. For the chest register especially he came to exactly the same conclusions as Musehold. We quote his new and enforced arguments:

"In stroboscopic observation the vocal cords are alternately seen completely closing the vocal slit, and lying one to one-and-a-half millimetres distant. Now in Figure 15 AB and CD may represent the top surface of the two vocal cords. According to the mechanism of reed pipes, if the vibration is supposed to be free, the free edges B and C should vibrate up and down in the direction of the two black arrows. To give an instance of the resulting width of the glottis, the length AB and BC may be supposed to be 10 mm. each, and the amplitude of the vibration in the direction of the black arrows to be 4 mm.; the distance of the two lips in the moment of extreme opening, as projected in the direction of laryngoscopy, then is still less than ¼ mm. Since the supposed amounts are already much too high, the impossibility of assuming vibrations of this mode is clear. In the way of the cushion mechanism, the vibrations have to occur in the direction of the dashed arrows. . . . For obtaining a maximum width of the glottis of two mm., in this case, the amplitude need only be 1 mm. . . . Unfortunately we are not able to ascertain whether in phonation the vocal lips have a cross section as thick as in Figure 13, but are forced to admit the possibility that the peculiar position of the muscular fibers in the vocal cords transform them into the shape of relatively thin bands at the moment of voice production. *But there is no positive argument for this assumption.* On the contrary, the fact that in the chest voice comparatively very little air is used, as we have already mentioned above, leads to the opposite conclusion, for this latter fact would be impossible in a mechanism of freely vibrating reeds. . . ."

To the description of the state of closure Nagel adds that after hitting together the cords are flattened and a part of the time and of the energy is used for that purpose.[33] On account of the softness of the vocal cords he admits the existence of a relatively small upward component of the vibration in agreement with Musehold (see dotted arrows in Fig. 15).

5. *The new models and latest publications.* The first artificial

[33] Nagel '08, pp. 736–738.

models of the new kind did not come out until 1913.[34] In that year Franz Wethlo, a pupil of *Gutzmann,* published his experiments with cushion-pipes, and shortly afterwards J. Richard Ewald published his article concerning the construction of cushion-pipes. Wethlo's model is represented by Fig. 16. It consists of a thick-walled glass tube of which by one cut straight across and two cuts in an angle of 45° the two pieces *BB* are cut out. Over these pieces rubber membranes *gg* (as shown in the figure) are stretched, and by means of a frame and the two screws *SS* every part is reassembled as in the original model, before it is cut into pieces. The two membranes, then, form two opposite air tight pockets or cushions, oblique at the bottom side, flat at the top side, the free edges of which show a rounded cross-section and form a rather wide glottis with the shape of a pointed ellipse. In order to close the glottis for phonation both pockets are blown up through little holes *p p*. Equal pressure in both pockets is obtained by using a Y-tube. The hole *m* is designed for the attachment of a manometer for the measurement of the "tracheal" pressure during phonation. For the determination

[34] Excellent presentations of the recent assumptions are given by Scripture '02, pp. 257–266, by Musehold '13, esp. pp. 114–134, which is wholly dedicated to the problem of voice production and contains the best existing laryngoscopic photographs of the voice lips in action, and by Panconcelli-Calzia '21, p. 66, etc. Shorter outlines but sufficient for a preliminary acquaintance are contained in Scripture '06 and '20, in Gutzman '09, pp. 31 f, in which the work of Koschlakoff, Réthi, Oertel, Ewald, and Musehold is briefly mentioned; also in Panconcelli '14 which gives a very instructive photograph of Wethlo's larynx model on p. 25. On the anatomy and physiology of the larynx, as far as it is involved in the voice production, the best orientation is found in the above works on phonetics: Scripture '02, pp. 229–250, Musehold '13, pp. 36–79. Most textbooks of general physiology written in English are surprisingly taciturn about the problem of voice production. In the book of Howell (which is referred to in West's Bibliography) the name of the vocal cords unfortunately is not even mentioned, and the larynx occurs only as an instrument for coughing. Brubaker's physiology contains a chapter on phonation and articulate speech, but does not touch our special question about the mode of vibration. This is also true for the textbooks of Starlin, Haliburton, and Hartridge, but these at least offer a cross-section of the voice lips which furnish a basis for theories concerning their possibilities of vibration. In the translation of Luciani's "Human Physiology" we found the only English book on physiology that states expressly that the vocal cords "vibrate synchronously". Likewise among phoneticians and acousticians writing in English, apparently only Scripture has dealt with the problem.

of the exact shape of the cushions Wethlo made beeswax casts after the method of Musehold ('13, pp. 120 f.), and obtained the cross-sections as shown in Fig. 17a with slack cushions, and those drawn in Fig. 17b with blown-up cushions. Wethlo used pipes 16 and 20 mm. width. In his pipes stretching of the rubber membranes was not the only means of raising the pitch. By mere increase of the air pressure within the cushions changes in pitch of about a fifth could be brought about. In stroboscopic observations of his pipe Wethlo found the synchronous upward and sideward movement which had been described by Merkel for the vocal cords. A phase of complete closure was observed; even when in the position of rest the opening of the glottis was wider than 1 mm. Sucking did not produce a tone but a valve-like permanent closure. Another interesting finding was that in this pipe increased tracheal pressure did not necessarily raise the pitch, but in a number of cases lowered it and in other cases did not change the pitch considerably. (Only in one case, which differed from the rest by having an exceedingly long and narrow air tube and requiring an exceptionally high air-pressure for phonation, was an increase in pressure accompanied by a rise in pitch.) If the same conditions were valid in the human larynx, Johannes Müller's famous theory of the compensation of forces would no longer be necessary.[35]

Ewald offers four different constructions of the cushion pipe which serve several experimental purposes. We confine our description to the one that has the closest similarity with the human larynx. This model has the advantage over Wethlo's model in that the width of the glottal slit and the air-pressure within the cushions can be varied separately and, therefore, has more possibilities of variation and is more similar to the real larynx. This is Ewald's description:

"Fig. 18 represents the pipe as seen from the side in half the natural size. It is made of brass, but the cushions are of rubber. The tube widens conically from bottom to top to which is fastened the box *b* that has the shape of an inverted sledge. Upon this the short piece of a tube *a* is attached which is intended for applying resonating cavities of any kind which modify or intensify

[35] Müller '39.

the tone in a manner similar to the action of the nasal and buccal cavities. But without such resonators the pipe also phonates very easily. The short tube just mentioned is seen from above as a circle in Fig. 19. It is soldered on the upper edges of the top box *b* and at both right and left sides touches the surfaces of the cushion pieces *c*. These pieces are pushed into the top box from the open right and left sides and carry the cushions which are shaded in the design. The pieces can be pushed in far enough so that the cushions touch each other and the glottal slit disappears, but they can also be fastened at any specific distance from one another. This is done by means of the small screws *s* which pass through slits in the lateral wall of the box *b* and screw into holes of the pieces *c*. In Fig. 18 on the left side the screw is taken away so that there the slit in *b* and behind it the hole in the piece *c* can be seen; the piece *c* itself is invisible since it is covered by *b*; on the right side the screw *s* is seen. The screw hole can also be seen in Fig. 21, the screws also in Fig. 19, the right one in the actual view from above, the left one as if the walls of the boxes were transparent. The shape of the cushion pieces which are pushed into *b* is represented in Fig. 20 in natural size in the views from above and from the side. They consist of two parts, the piece *c* which is pushed into the top box *b* (*cf.* Fig. 19) and the piece *e* which fits exactly into the hollow side of *c* and is screwed tight by the piece *d*. A separate design of the piece *e* is shown in Fig. 21. This piece *e* is wrapped with the rubber membrane that is to be the cushion, as is seen in the cross-section Fig. 20b where the membrane is represented by a thick black line. In this cross-section it should be noticed that the neighboring edges of the rubber membrane do not touch each other but leave a small aperture between them. In no case ought the edges to overlap, as this would make impossible a perfectly tight closure between the pieces *e* and *c*. The rubber membrane then is fastened by silk threads which are held in place by the two grooves seen in Fig. 21. The knots should not remain, of course, between *e* and *c*. Before fastening the rubber membrane, the surface of *e*, as far as it comes into contact with the membrane (*i.e.*, also the grooves), is painted one or more times with rubber cement of the consistency of syrup.[36] The cushion formed by the rubber membrane in this manner encloses an air chamber which communicates with the outer space only through the piece *e*. The piece *e* then is put into *c*, and the tube *d* which has threads inside is tightly screwed on it; hereby *e* is pressed against *c* and an air-tight closure between them is obtained. . . . By pressing air into the rubber tubes connected with the pipes *d*, the cushions may be given any degree of tension and rigidity. . . . Equal pressure may be obtained by using a Y-tube. . . . According to the actual problem the size and the shape of the cushions, the thickness of the membranes, the shape and the width of the glottal slit, etc., can be varied independently without changing the principle of construction. . . ."

Ewald tries to answer the question whether his cushion-pipes represent a special type of pipe or just a new kind of membrane pipe. If they vibrate transversely by bending up and down, they are nothing but excessively thick membranes; only if they

[36] Our own experience led us to put first a good layer of household cement (dissolved celluloid) on all brass parts which came into contact with the rubber, because rubber which was in contact with brass deteriorized in a few hours.

vibrate by being rhythmically compressed and thus yielding sidewise, do they represent a different type. By the application of tiny levers Ewald found that there is really some up and down movement left in his rubber model, but the sidewise movements were considerably stronger. He explains this by the deformation that the rubber cushion suffers through the increased air pressure from below. He then constructed two models in which only strictly lateral movements were possible and in which the cushions themselves were rigid and thus could not be deformed. In one of them the two rigid bodies forming the glottis were compressed by the action of two rubber membranes which were applied outside and the free movements of which were made aperiodic; thus, if periodic movements of the bodies occurred, they could not be due to the period of these membranes. In the other of these models there were no elastic parts at all, as the compression of the bodies that formed the glottis was obtained by the action of electromagnetic coils. With both pipes tones could easily be produced. Hereby, Ewald proved that the cushion-pipe is no mere modification of the membrane pipes, but that there is in fact a different principle operative in their way of opening and closing the vocal slit. Ewald's larynx model is the most perfect one known to the present writer. In its original form its only disadvantage as compared with the model of Wethlo is that it does not allow for direct inspection, *e.g.,* for the stroboscopic study of the cushion action. But this could easily be effected by making the whole side walls of the top box or parts of them of glass, or better, of transparent celluloid.

In different publications Scripture had reasoned from the speech curves to a puff action and from this to the cushion mechanism. The year 1914 brings a much too little known paper of *Otto Weiss* [37] in which by new cadaver experiments the actual coexistence of these processes is presented to the eye. Weiss made his experiments with calf larynxes, the vocal cords of which were tensed by compression from the side. He recorded at once the subsequent widths of the glottis, the air pressure below the

[37] Weiss '14 II.

cords in the trachea, and the air pressure above the cords in the open air. His curves show: (1) each full vibration of the cords makes one opening of the glottis; (2) when the resting cords close the glottis, the glottis may be open during vibration for 1/5–1/20 of each period, according to the tension of the cords; (3) when in the state of rest the glottis is about 1 mm. wide, the glottis still closes during phonation for half of the period with relatively high tension; (4) with lower tension the closure finally disappears. The curves of tracheal pressure closely correspond to the curves of opening. They show a sudden decrease of pressure during each opening which represents the puff as seen from the back side. The pressure curves of the outside air represent typical frictional sinusoids of high frequency without any similarity to the tracheal and glottal curves, except the frequency of stimulation. This is the simplest possible proof of the puff theory, and at the same time it shows evidence of the identity of the laryngeal and of the perceived "fundamental." This identity was also verified by *Tonndorf* in a very careful analysis of the action of the stroboscope in laryngoscopy in 1926. The experiments of Weiss thus show that even the dead larynx (at least that of a calf) does not produce free vibrations, as Ewald had been led to suspect by the reports of Oertel, *etc.*, but that it, in principle, acts as do the cushion-pipe of Ewald and the living human larynx according to Musehold and Scripture. They also give the very interesting result that the principle of action is not changed when the cords vibrate without complete closure; and, also, that the vibration is not necessarily altered when the tension of the two cords is not exactly the same, so that the curves of opening are not perfectly symmetrical.

The next few years are remarkable in that no new theories concerning the mode of vibration of the vocal cords are to be found. During this period better methods of laryngoscopic observation were developed than before. Above all, stereoscopy and stroboscopic stereophotography were much improved, especially through the studies of *Julius Hegener*.[38] As we remember,

[38] For the development of the most important mode of inspection, the stereoscopy and stereoscopic photography, see above, all Hegener '21.

the older laryngoscopic observations had been seriously doubted for their lack of stereoscopic vision.[39] By the introduction of improved stereoscopic methods these doubts would necessarily have yielded to definite statements of possible errors. But Hegener says concerning the results obtained with his devices: "The original purpose of recognizing small differences of height and of getting a tridimensional view of the larynx has . . . been completely achieved. It is already certain that several things will have to be interpreted differently. . . . For instance, in cases of satisfactory voice formation sometimes individual differences in the height of the vocal cords during phonation can be observed which thus far were not known to exist. In so-called phonastheny we found habitual differences of several millimeters in height, in cases where to former inspection it had seemed as if only the closure were defective in consequence of a wrong use. . . ." But Garcia's theory, he states, is still well established and unaffected by his stereoscopic observation.

The brief paper of Scripture in 1920 has already been mentioned. It may be added that it contains the most pregnant and clearest presentation of modern assumptions concerning the voice registers which, at least for the head register, are still problematical. How problematical the theory of head tones has been until recently is shown by the fact that as late as 1907 and 1909 two well known laryngologists[40] had doubts as to whether in this register the voice is really produced by vibrations of the vocal cords and not perhaps by a kind of flute mechanism.

In 1925 Tonndorf raised the question as to whether in the cushion-pipes the vibration is maintained as long as the air passes through the slit. He asks why after some time with a steady opening of a certain width a state of equilibrium is not reached between the opening force of the air-pressure and the closing force of the elasticity of the cushion. And as this actually does not occur, he concludes that there must be a special force which assists the elasticity only during the state of wider opening. This

[39] Chiefly by Nagel '08.
[40] Nagel '07 and Katzenstein '09.

force, according to Tonndorf, is to be found in the negative hydrodynamic pressure which always comes about when air passes through a narrow opening into free space. Thus, when the slit is too narrow or completely closed, the hydrostatic pressure in the trachea increases and by its wedge effect widens the slit. Then a vigorous jet of air passes through the slit and by its negative hydrodynamic pressure, in addition to the elastic forces, the slit is contracted again. Thus, a state of steady equilibrium can never be reached and the vibration must necessarily continue. A number of experiments seem to show that this process of vibration comes about, also, with perfectly inelastic material, as strips of paper, as should be expected if the theory is right. But this can not be considered as definitely established.

In 1926 Tonndorf doubted the observation of Musehold that in the chest register each vibration contains a long period of closure of the glottis, and that in the head register the glottis is open continuously. But, as his statements are supported only by his own observations without any records, we shall have to suspend judgment until the moving pictures of the functioning larynx made by G. Panconcelli-Calzia are known, the first section of which was shown in the summer of 1927 and contains records of different rubber models and of the mouth-lips of a trumpet-blower.[41]

The same year (1926) brings two new, or renewed, theories at once. We shall first consider briefly the theory of *Weleminsky*. He starts from Musehold's observations, just mentioned, that in the chest register the movements of the vocal cords consist of lateral "half vibrations" (since they touch in the middle). Moreover, they do not go on continuously but are interrupted by long periods of closure. Weleminsky thinks such vibrations do not agree with the laws of physics. He finds still another dark spot in the current theory. "Suppose the stream of expiration could really act upon the voice-lips so that a movement in the

[41] For the kinematography of laryngeal movements see Chevroton, Hegener '13, and Panconcelli '20 and '27.

horizontal plain developed . . . , on the return this movement would stop in the median position" and would never pass beyond it, if the vocal muscle were to be the vibrator, " since in the state of contraction no muscle is capable of free vibration." We find here, again, the argument of Fournié, but Weleminsky, who apparently does not know Fournié's work, makes a conclusion similar to that of Fournié but only in connection with the head register in reassuming the old theory that the edges of the vocal cords here swing up and down without a participation of the muscle in the movement. For the chest register he offers the interesting assumption that the real vibrator is the so-called *conus elasticus,* that is, the elastic membrane that covers the inside of the larynx, the lower end of which is attached to the cricoid cartilage, the upper end to the vocal muscles, and which thus forms the real " vocal bands." The vibrations of this membrane supposed by him are explained exactly in the manner in which Garcia explains the movements of the cords themselves. The movement of the vocal cords, then, is only a fragmentary participation, a partial accompanying of the real elastic vibration of this internal membrane (Fig. 22). Of course, the theory of Weleminsky which he tries to support by an extensive discussion of the aspect of the different mechanisms of pitch control, under his new assumption, can not *a priori* be refuted. It may only be stated that, as the existence and the theory of striking reeds prove, " half " and in addition " interrupted " vibrations are not so foreign to physics and are not so difficult to explain as he supposes. And secondly, if Tonndorf's theory of the lateral vibrations is true, no purely elastic vibrations are required in order to have the vocal cords pass beyond the median position on the return swing. Their degree of elasticity is, therefore, of little consequence. Apparently laryngoscopic observations do not help to solve this question, since it is necessary to observe the cavity below the glottis for this purpose. More exactly, frontal views of the larynx during phonation would be necessary. At present only such views taken during the state of inspiration

are available in the X-ray photographs of A. Réthi.[42] We shall refer to these later.

The second new theory of 1926 concerning the voice was proposed by *Robert West.* The next chapter is dedicated to a careful discussion of this theory and its foundation.

II. *Experimental Investigation*

6. *Analysis of the main argument.* West's study was initiated by the observation of a fact which seemed to require the assumption of alternate vibration of the vocal cords. Our first question is whether his hypothesis is necessary to account for the fact observed by him. The intention of West was " to record movements of the larynx without relying upon the air currents delivered at the mouth or nose" (p. 246). He, therefore, recorded the vibrations of the point of the thyroid cartilage, *i.e.,* of the "Adam's apple" by a graphical method which we shall assume as sufficiently reliable for the purposes of the present analysis. " When the curves of the experimenter's own laryngeal movements were first produced it was at once evident that for every wave of what was perceived as the fundamental, there were two laryngeal movements, *i.e.,* if the voice were made to sound in unison with a fork of a frequency of 128 dv., the curve would show two " humps," or maxima, for each vibration" (p. 250). Fig. 23 shows an instance of West's findings. Here is the fact that calls for explanation.

West's next step, then, was to take traces from the walls of a rubber model, which was identical with Johannes Müller's model (Fig. 1), during vibration. Here he obtained similar curves as from the "Adam's apple." Since the stroboscope revealed that the vibrations of this model were alternate, he assumed that this was the cause of the double curve and that, therefore, the double curve obtained from the outside of the larynx was also caused by alternate vibrations of the vocal cords.

[42] The photographs are contained in Réthi I '13, a picture of his improved slide in Réthi I '14. X-ray photographs of the vocal cords have also been taken by Russel at Ohio State after an improved method, but his work has not been published at the date of this writing.

Our question is: Is there any necessary connection between alternate vibrations of two membranes forming a glottis and double vibrations of the walls of the pipe? If we consider the traces obtained by West a sharper formulation will prove necessary. In no case is a full vibration of the vocal cords or of the rubber membrane represented by two exactly equal vibrations of the wall, but by a pair of vibrations which differ in amplitude as well as in duration.[43] So we have to ask whether there is a necessary connection between alternate vibration of the cords and such specific unsymmetrical double vibrations of the walls as were observed by West.

The forces acting upon the walls of the box during vibration are (1) the tension of the membranes at the points of their attachment and (2) the pressure of the air enclosed.

We first consider the possible influence of the tension. Every vibrator that is stretched between two points of attachment, when swinging from one extreme position to the other and back, passes twice through the middle position of equilibrium where it is shorter and, therefore, less tense than in any other position. Thus, during one full vibration it pulls twice at the points of attachment.[44] How about two parallel vibrators as the vocal cords are? Since the actual direction of the movement is without importance for the tension, the effect is exactly the same whether the two vibrators are in phase or 180° out of phase. With other phase differences than 0° and 180° there are four moments of highest tension and four moments of lowest tension during one full vibration of two parallel vibrators. If tension is involved in the phenomenon of double vibrations of the outside of the larynx or any similar pipe, it does not make any difference whether the vibrations are synchronous or alternate. West himself found that the influence of tension had no practical importance in the artificial larynx, as he found no change in the

[43] Waves doubled so symmetrically that "humps of equal size and shape" result and are only observed in the manometric flame by West (pp. 258 f.); now it is generally admitted that the manometric flame is not reliable enough in finer details, so we rely only on West's tracings which present but unsymmetrical pairs.

[44] Barton '07, '10, and Raman '11.

characteristics of the curve produced when he recorded the movements at various points on the surface. Contrary to the air pressure, the tension should cause a distortion of the body of the pipe; that is, the amplitudes should vary from point to point along its surface, which was not the case in his model. However, West did not investigate more than one point of the human larynx so the possibility of distortion is not yet disproved there. Still he is right in assuming that the tension is not the cause of the double vibration observed by him; for with neither alternate nor synchronous vibration the observed difference in the amplitude and duration of two subsequent vibrations would occur in this case.

The possible influence of the air pressure upon the walls of the pipe will be considered under three different arrangements of the membranes, each under the assumption of alternate as well as of synchronous vibration (Figs. 24–26).

First arrangement (Helmholtz's model): The membranes meet in their lowest position in an angle of, for instance, 90°, if their vibrations are synchronous. If their vibrations are alternate, they are free vibrations; but we assume that their inward movement does not go considerably lower than the meeting point mentioned above.

Second arrangement (J. Müller's and West's model): The position of equilibrium is at an angle of 180°; only free vibrations occur, and the inward movement is of the same amount as the outward movement.

Third arrangement: The position of equilibrium is at an angle a little smaller than 180°. This is the real state of an intentionally flat model, as West's during action; the pressure inside is greater than outside so the vibration is still free, but the inward movement is relatively smaller than the outward movement.

In order to understand the three pairs of representative cases offered in Fig. 24 we have to remember W. Weber's principle of pipe action, according to which the air vibrations depend exclusively on the subsequent changes of the width of the passage offered to the air in reed pipes. It is justifiable, therefore,

to measure the changes of air pressure outside of the passage by the width of the passage. If we invert the curve obtained this way, we get the curve of the pressures acting on the walls of the pipe from the inside, in a first approximation. In our figure the dotted lines show the subsequent widths of the passage for one full vibration of the membranes.

We will consider, first, the alternate cases 1a, 2a, and 3a. In 1a the opening is twice wider and twice smaller during one full vibration; but the duration as well as the size of the two periods is exactly alike, in contradiction to West's tracings. Moreover, the periodic change of the opening is so slight that it is hard to understand how a reasonable tone can be produced without an enormous waste of breath. In 2a we find two sudden complete closures for one vibration. Here the periodic change is sufficient for voice production; but, as before, the two periods are exactly alike and thus do not fit West's tracings. Moreover, the curve is not sinusoidal, but this is a minor difficulty. Case 3a does not require a separate treatment as it is, but an intermediate case between cases 1a and 2a. All of the three cases, therefore, account for a double pressure vibration but not for the difference that is always found between two subsequent humps. We will try to account for this difference by assuming certain asymmetries. First, the vibrations of the two membranes might not be exactly alternate but not quite opposite in phase. By a construction like Fig. 24 it can be shown that in none of the three arrangements would this cause a difference in the size of the humps. Second, the amplitudes of the two membranes might not be equal. Since in the first arrangement the amplitude is so small as we found, such secondary differences as we are considering now must be of zero order; therefore, we shall limit our consideration to 2a and 3a. Fig. 27a shows that in 2a different amplitudes of the two vibrators do not make the humps different. But there is a difference in 3a. Fig. 27b shows the extreme case in which one of the membranes is not vibrating at all. But as soon as we consider possible cases instead of such an exaggerated fiction this difference is surprisingly small. Let the left vibrator

in our figure have an amplitude of, say, 2/3 the amplitude of the right vibrator. Then, a simple calculation shows that the difference between two subsequent openings is not more than 1/50 of the wider one. The difference in duration, of course, is similarly small. The differences found in West's tracings are without exception much greater than that. Thus, the assumption of a difference in amplitude does in no arrangement give the desired result, namely, pairs of pressure vibrations of different duration and amplitude. However, this kind of asymmetry is only of a special theoretical interest. Its extreme form, in which one of the membranes stands completely still and only the other one vibrates, represents nothing but an ordinary reed pipe with a single freely vibrating reed. This shows at once that in order to produce double pressure vibrations by single elastic vibrations it is not necessary to have two vibrators and a resonator " responding to both " of them, as West seems to think (p. 260), but that a single vibrator can have the same effect equally well. In fact, freely vibrating reeds, whether metallic or membranous, always have a predominant second partial. In a study of *Raps* this was observed with single metallic reeds. Otto Weiss who experimented with single membranous reeds[45] obtained curves that are surprisingly similar to West's curves; and he did not succeed in eliminating the dominance of the second partial by any change in the resonant condition of his pipe.

There is, however, one kind of asymmetry left in which, with alternate vibration, pairs of different pressure vibrations are caused. This is the case when the position of equilibrium is not the same for the two vibrators as in the exaggerated instance of Fig. 28a where the left reed moves round the flat position, the right one round a position of 45°. The two dotted lines represent the maximum openings; the small horizontal line in the middle represents the smallest possible opening. Even under such conditions as are fulfilled in a real vibration process, the ratios between the two maxima are such as are required by the curves of West as Fig. 28b shows, where the one maximum *AD*

[45] Weiss, '14 I.

is almost twice the other *BC*, the minimum approximates zero, and the duration of the two part periods is similarly different. With this kind of asymmetry other kinds may be combined without much changing the outcome, for instance, a difference in amplitude. In the extreme case, where only one of the bodies moves, we have a kind of pipe which is of interest in the building of a harmonium, namely, a pipe with one free reed that does not produce an equally large part of its vibration at either side of its position of smallest opening. Thus, we found one arrangement of double reeds in which an alternate vibration results in a pressure curve of the shape found by West; but we found at once that the vibration of a single reed can have exactly the same effect. It is easy to see how the conditions of this arrangement can unintentionally be fulfilled by using two flat membranes. When the membranes are in motion, that is, when air escapes under pressure through the slit between them, their position of equilibrium round which they vibrate is naturally not the same as their position of rest, but is adapted to the new condition, the difference of pressures inside and outside the slit. Suppose, now, that their tensions are not equal. Then, the position of equilibrium between the air pressure and the elasticity will be farther out of adjustment for the looser membrane than for the tenser membrane. Besides, the amplitude of the looser membrane will be greater than of the tenser one (Fig. 28b). This is exactly what is required to account for the tracings taken by West from the walls of his pipe. Since he did not check the relative tension of the two membranes (the halves of his rubber cap) there is much more chance that their tension was different than that it was equal. So we can suppose with high probability that we found the exact connection between the vibration observed on his model and the corresponding vibration curves taken from its walls.

After we found a condition under which alternate vibration yields unsymmetrical double pressure curves, the question remains whether only alternate vibration can yield such curves. We consider again the three representative arrangements, this time

with synchronous vibration; that is, (1) Helmholtz's model (Fig. 1b) shows one opening and one closure during one vibration of the membrances. We, therefore, leave it out of account. (2) (Fig. 2b) the model of J. Müller and West, shows two openings and two closures during one vibration exactly as do flat membranes when vibrating alternately; but the two openings are still identical, contradictory to West's tracings. (3) (Fig. 3b) finally shows all features necessary for explaining a double pressure curve of the shape that was found by West. It is obvious that according to the angle of the position of equilibrium any ratio between the two partial vibrations can be obtained from identity to complete disappearance of one of them. The effect of any asymmetry would be more or less to diminish and finally to cancel the difference between the two partial periods in this arrangement. It is remarkable that in J. Müller's and West's model during action the position of equilibrium will not be that of arrangement 2, but that of arrangement 3 because of the difference of pressure inside and outside of the membranes which was mentioned before. For this consideration has the interesting result that West would most probably have found just the same kind of double humps as he obtained with alternate vibration, if the membranes had been vibrating synchronously. According to the records of Simanowsky (36), this would have been the case if the tensions of the two membranes had been equal and the membranes had been perfectly symmetrical in every other respect.

We have found so far that it is not necessary to assume alternate vibrations in order to account for double humps, but that there are equal chances for their occurrence with synchronous vibration. We have limited our discussion, however, to vibrating membranes. What are the chances if the vibrators are cushions or lips, *i.e.,* if they can be compressed sidewise as well as bent up and down? Suppose the two lips bounce together in the direction of the arrows in Fig. 29, as, according to Ewald '98 and '13, the vocal cords do? What is the effect if they have some elasticity? It is at least possible that they rebound in the direction of the dotted arrows; the resultant vibration would then

pass in the way designated in Fig. 30, as the small arrows indicate, and would bring about two maximal openings of different size during one full vibration of the lips. Thus, we find here again all that is required to account for West's tracings without assuming alternate vibration.

The result of our analysis is that double vibrations of the walls of a vibrator similar to the larynx are possible under three different conditions, one of which involves alternate vibration but two of which involve synchronous vibration of different character. It is, therefore, not justifiable to reason from double vibrations of the outside of the larynx to alternate vibrations of the vocal cords.

7. *The stroboscope.* Are there any other arguments left for assuming alternate vibration of the vocal cords? Yes, in the first place, that in a pipe of similar construction, in Müller's and West's model, the two vibrators also move alternately.[46] This argument must also be analyzed carefully. The analysis will have to answer two chief questions: (1) Does the model really move as it seems through the stroboscope? (2) If it does, is the model sufficiently similar to the real larynx to allow for the analogy?

The first question seems to be idle, for, since the work of Koschlakoff and Simanowski, it seems to be settled that flat double membranes vibrate alternately if the tension is not very carefully equalized. But there still are contradictions. With membranes vibrating without interference, Koschlakoff '84 never found nodes dividing the edges, whereas West found nodes which moved along the edges (p. 253). In larynxes of normal cadavers Koschlakoff '86 found only total and synchronous vibration; while to West the edges of a cadaver's larynx showed the same characteristics of vibration as his artificial larynx, that is, alternate and nodal vibration (p. 254). The mouth-lips of a trumpet-blower were also seen by him to vibrate similarly, whereas Musehold '97 and Panconcelli-Calzia '27 here again

[46] West offers a number of other arguments but none of them really require the assumption of alternate vibration. Cf. p. 263 of his article.

found synchronous vibration. Since the conditions of the observed objects, as far as can be judged, were the same when these contradictory results were obtained, it must be suspected that the stroboscope was the cause of the differences. The purpose of this section, therefore, is a careful analysis of the stroboscope as to its sources of error and the conditions most favorable to observation. This will enable us to judge the relative reliability of the observations in question.

Among the different stroboscopes or devices for periodic exposure the rotating disc with exposure slits was found to be most favorable for our purposes. This device has not a perfect constancy of speed, but its advantages are quick adaptability of speed, sharp cuts between exposure and interval, arbitrary relative length of exposure, and better lighting.

Quick adaptability is of much greater value than perfect steadiness, for it is known that the slightest change in air pressure immediately modifies the frequency in membrane pipes so that the stroboscope has to be retuned continuously during observation. When connected with a color wheel motor the speed of the disc can be modified almost instantly by the resistance coil mounted with the motor. The steadiness of the motor is, under ordinary conditions, greatest with high speeds. But, on the other hand, the higher the speed the harder it is to make quick delicate adjustments of speed. And, moreover, using high speeds would involve the use of small discs with few slits; with such discs " octave errors " of the adjustment can hardly be avoided.[47] We, therefore, preferred low speeds. Since under this condition large discs must be used, the loss in steadiness can not be so great as otherwise. By the larger size of the disc the air friction is increased, the steadying effect of which is generally known. Octave errors were excluded in the following way: A cardboard disc about 50 cm. in diameter was used, the air friction of which reduced the speed of the motor to 2–20 revolutions per sec. Since it was mostly frequencies of 100–200 d.v. that were to be observed, thirty-two radial slits were cut along the periphery.

[47] West's statements on p. 251.

For an observed frequency of 160 d.v. the stroboscope had to make exactly five revolutions per second, which could be counted easily by a mark made on the disc. The simplest means of measuring the speed was to use the disc as a siren; that is, to blow through a small glass tube against the row of slits and to listen to the pitch. To count the revolutions by the mark was only a means of checking the octave, for instance, whether the perceived " c " had really a frequency of four times thirty-two and not one of eight times thirty-two.

The relative length of the exposures has to be considered. Sharpness of the apparent objects is the first requirement of stroboscopic observation. Theoretically only a point out of every whole period of vibration should be made visible by the slits. But, practically, it is not possible to cut down the relative length of exposure under a certain limit without making the field of vision so dark or foggy that the observation becomes uncertain and finally impossible. Figs. 31–33 illustrate the requirements of sharpness. The abscissa represent the time, the ordinates the amplitude, that is, all the different positions of the vibrator which are possible. We have to bear in mind that all positions that are exposed subsequently by one and the same slit appear simultaneously to the eye. If the amplitude is as wide as *AB,* observation with the naked eye shows a foggy mass as wide as *AB.* Stroboscopic exposures of the duration *CD, C'D'* . . . . , that is, of one-third the period, for instance, may still produce a foggy mass of the width *EF,* which means that the observation is hardly improved. It is clear that the parts of the vibration curve decisive for the choice of the length of exposure are the steep parts, which represent the fastest moments of the movement. By exposing a slower third of the period the apparent picture becomes only as wide as *IK.* A glance at Fig. 33 shows that, at the point of fastest movement, an exposure of one-sixteenth of the period still yields a picture about as wide as one-fifth of the whole amplitude. So we preferred to make the exposure slits as wide as 1/32 of their distance.[48] The field of vision is already

[48] Afterwards we found in the literature that 1/20 is considered short enough for finer laryngoscopic observations by the most cautious investigators.

rather dark with the ratio used; but it could be made perfectly clear by putting a 40 watt bulb directly above the object [49] and avoiding a stimulation of the eye during the intervals. For the latter purpose a disc was used that was black on the side which the observer faced. Putting the object into the sunlight and the stroboscope in the shade had the same effect.

The last remark implied that the stroboscope was put between the observer and the object in such a way as to cut off the line of vision directly. In clinical laryngoscopy the disc is generally put between the light and the object, or, more exactly, between the light and the head mirror, which naturally does not change the result. This arrangement is indispensable for the observation of the larynx, for here the stroboscope, if put in the line of vision, would also constantly cut off most of the light that is reflected into the throat by the head mirror. The first method used to avoid this was to apply the mirror in front of the disc; that is, to have the rotating disc between the forehead and the mirror. This method, of course, makes much more trouble than cutting off the light stroboscopically on its way from the bulb to the head mirror. But since no head mirror is used in our observations neither arrangement is necessary.

As was mentioned above, it is easier to adjust the stroboscope to fractional parts of vibrations than to the right octave. As is known, the stroboscope is optically " in tune " when the observed vibrator or rotating body apparently stands still. But this is not only the case when the frequency of exposure is the same as the frequency of the periodic movement, but theoretically, also, when it is 1/2, 1/3, 1/4, . . . of it, and, moreover, with certain restrictions, even when it is 2/1, 2/3, 2/5, . . . of it, as Fig. 34 shows. In our observations only the relations 1/2, 1/3, 2/1, and 2/3 are worth considering.[50] One would think that there would be no problem where the observed object is a musical instrument and the stroboscope is built in such a manner that it can be used as a siren whose revolutions can be counted. The

[49] This proved to allow for a much easier observation than to light the interior of the pipes, as West did (p. 252).

[50] Other ratios no longer yield clear pictures and, therefore, cannot mislead.

tones of either one need only to be compared to see whether they are equal or not. In case of doubt the revolutions of the disc could be counted. This would be all right if the fact of the "subjective fundamental" is disregarded; *viz.*, when two tone series of different timbre are compared, tones with the same objective fundamental sometimes seem to be an octave apart, while other tones, the objective fundamentals of which are really an octave apart, seem to have the same pitch. Correspondingly, if the objective fundamentals have a ratio of 2/3, it is often not clear to the ear whether the second tone is the higher fifth or the lower fourth of the first one. But this problem can be solved. As we know, when the stroboscope is optically in tune the object is seen to stand still in the position that happens to be caught by the slits. When the stroboscope is slightly out of tune "each successive exposure will catch a slightly different phase of the oscillation or revolution and the objects studied will appear to move slowly; if the speed of the stroboscope is such that the period between successive exposures is slightly shorter than the period of the movement of the object studied, it will appear to move backward; if the opposite is true of their relative speeds it will appear to move forward" (West, p. 251). This is equally true when the stroboscope has 1/2 or 1/3 of the frequency of the vibrator, with the only difference that the exposures are relatively larger and the object appears correspondingly dimmer, according to our above consideration. What happens when the disc has the double frequency of the vibrator, *e.g.*, of one of our membranes? Fig. 35 shows that only the middle position can appear clearly. When other positions are caught a higher and a lower one are always caught alternately and, therefore, must be seen at once. Thus, instead of slowly moving up and down, each membrane must appear to split into two membranes; it must look like a folded sheet of transparent paper which slowly folds and unfolds. When there are two vibrating membranes in a position where they must be expected to move alternately, this apparent process of splitting, or of folding and unfolding, must be synchronous. This is exactly what is sometimes observed. Herein

we have a simple means of checking the octave. When, according to its siren tone, the stroboscope is approximately in tune with the vibrator, we raise its frequency to the next octave. If, then, we get the splitting phenomenon, we know that the frequencies were equal before. If the splitting phenomenon does not occur we raise the frequency of the stroboscope another octave until the phenomenon is seen. Then we have just to go back to the next lower octave and know that this is the objective fundamental of the vibrator.[51]

Perhaps the worst disadvantage of the disc stroboscope in its primitive form is that it does not expose the whole field of vision at the same moment, but subsequently, and thus necessarily, distorts the apparent shape of the object. This is equally true whether the line of vision or the rays of the lighting bulb are interrupted.[52] In our experiments it was observed that sometimes the vibrations of the membranes were in waves wandering back and forth along the edges, quite as West described it; but sometimes there were only total vibrations and the opening was distinctly a pointed ellipse, as was expected from the observation with the naked eye, without any nodes, movements along the edges, or asymmetry. It was soon discovered that the wavy aspect disappears only when the edges of the membranes are situated in a plane that is formed by the eye of the observer and the slit of the stroboscope at the moment of exposure. In Fig. 36 this plane is identical with the surface of the paper. If, in the figure, the edges do not lie in the surface of the paper, the wavy vibration appears and becomes strongest when the edges are perpendicular to the surface. In other words, the vibration appears to be simple and total if the whole length of the vibrators is exposed approximately at once through every slit. It appears to be wave-like and asymmetrical if the slits expose different points of the vibrators one after another. The nodal movement, then, is a mere stroboscopic illusion. Let us consider its origin by means of a simple case. Suppose the vibrating edge is about

[51] Another simple method of checking the octave will be found at the end of this section.

[52] Cf. Muschold '13, p. 108.

as long as the distance between two slits on the stroboscopic disc. It lies at right angles to the exposing slit and to the line of regard. In Fig. 37 the slits are perpendicular to the surface of the paper; they move in the direction of the arrows and the vibrating edge is extended, for instance, between *a* and *b*. When the disc and the vibrator are in tune the second slit must arrive at the original place of the first one exactly after one whole vibration is made. That means that in this time it must have exposed all the points of the vibrator between *a* and *b* in subsequent phases of one whole vibration; that is, the vibrator must be seen in wave form. If the instruments are in tune this wave figure must appear in a state of rest; if the stroboscope is faster, the waves must appear to move in a direction opposite to its direction; if the stroboscope is slower they must appear to move in the same direction as it moves. All of this proved to be the case. The stroboscopic distortion can be eliminated completely by focussing the rays of the lighting bulb through one or two lenses and placing the stroboscopic disc exactly at the focus. For finer observation of two-dimensional or tridimensional vibrators this is the only safe method. But as was mentioned above, for the observation of one-dimensional vibrators, as the edges of our membranes are, a practically simultaneous exposure can be obtained by a suitable relative position of the stroboscope, the object, and the observer. The most favorable position is shown in Fig. 36, II, with the edges of the membranes perpendicular to the slits but almost parallel to the line of regard. In this position the whole period of both could be observed simultaneously without any mutual disturbance. Eventual asymmetries, as unequal amplitudes or lagging of one membrane, could only then be seen. Of course, the distortion came back when objects with a vertical extension were observed, for instance, pipes with roof-shaped caps. The illusive waves, then, moved up and down between the free edges of the membranes and the lower parts of the top edge of the tube. But since the cause was known it was possible through calculation to eliminate the illusive traits of the movement.

It may be added that another simple method of checking the

octaves can be developed from the stroboscopic distortion. In Fig. 37 it is easy to see that, if the edges of the vibrators lie at right angles to the exposure slit, two small waves must appear if the disc has only half the intended speed, etc. In fact, when the pipe is observed in this position while the stroboscope is being started, it is interesting to see how, first, a whole chain of little waves or openings appears and, while shifting back and forth, these waves gradually decrease in number until only a single wave is left when the speed is correct.

As to the questions put in the beginning of this section, we may summarize the answers suggested by the analysis of the stroboscopic action, as follows:

1. There is no doubt that West's model had alternate vibration.

2. West's observation of nodes travelling along the edges of the membranes was due to stroboscopic distortion; the real vibrations under all conditions observed by him are total, as was generally assumed since 1896.

3. Since West had no method of checking the octave error or any other stroboscopic illusion, while the other observers had such methods, his observations of alternate vibration in mouth lips and in a cadaver's larynx are not reliable enough to disprove the numerous contradictions of earlier investigators.

8. *The action of different models.* After it is certain that in West's model the membranes vibrate alternately, the second question remains whether this model is sufficiently similar to the larynx to allow for the analogy. In the first place this question does not concern the similarity of material, provided the difference in elasticity is not too great, but concerns the similarity of shape and arrangement.[53] In fact, a pair of flat membranes does not look very similar to the vocal cords, according to the cuts of Fränkel. However, it can not be maintained *a priori* that this difference of shape necessarily makes a difference in the mode of vibration. What, then, is the status of the question? In order to make the model more similar to nature Helmholtz,

[53] Strangely, West is only concerned with the question of material and entirely overlooks the question of shape and arrangement. (p. 253.)

in 1862, replaced the flat membranes by oblique ones; Koschlakoff in 1884 replaced the round tube by an elliptical one; and Wethlo and Ewald in 1914 replaced the membranes by cushions. The last shape has been unanimously considered by modern laryngologists as the closest reproduction of the human larynx. All of these models, from Helmholtz's to Ewald's, have synchronous vibration. Thus, the difference in shape seems to have consequences that are fatal to West's conclusion. But possibilities still remain. In this section we are only concerned with the question how safe it is to reason from the behavior of certain models to the behavior of the real larynx. Therefore, we leave out of consideration the results of direct observation of the action of the larynx which were presented in I, 5. West assumes correctly that there is a causal connection between asymmetry and alternate vibration. In fact, with perfect symmetry in no pipe with double vibrators does alternate movement occur. He is further right in assuming that the human larynx is more likely to be asymmetrical than symmetrical.[54]

[54] It may be briefly considered in which special way the asymmetry might act towards alternate vibration. West makes two different assumptions about this way. One (p. 254) in which the exact alternate formation and breaking away of eddies at the two sides of the air passage is believed to cause the alternate vibration of the membranes. Another (p. 256) in which a possible difference in the tension of the two membranes immediately causes asymmetry of pressure which is likely to shift from side to side. By a series of observations of water streaming through a narrow slit, Helmholtz's statement was verified according to which "it is not until it reaches a distance of some centimeters that the outpouring sheet splits up into eddies or vortices . . ." (Cf. also the remarks of Wachsmuth, Mallock, and Lunn, about the distance in which the jet of air begins to be curled, *i.e.*, to break up into eddies, about its stiffness in the next environment of the slit, and the *vena contracta* effect which is such as to prevent all pressure of eventual eddies in a direction perpendicular to the surfaces of the blocking sheets.) It was found, moreover, that these eddies are microscopical at the beginning and grow to a considerable size only in several times the distance of their first appearance, where they can no longer have the influence on the walls of the dam which is a necessary condition of West's hypothesis. In the place where, according to West, the eddies should form, two steady circular streams which never break off can be observed. Besides this it was not possible to find a regular shifting of the jet from side to side: woolen threads attached at the two sides of the slit moved once in the same, once in opposite directions. Experiments made with the intention of finding the real cause of the alternation pointed in the direction of West's second assumption. Slight asymmetries of the position of the two parts of the dam caused considerable asymmetries of pressure apparently in such

The question is now whether asymmetries, and what asymmetries, are capable of changing the synchronous vibration of the aforementioned pipes into alternate vibration and, if so, how great the asymmetries must be to have this effect. This question has apparently not yet been put for the newest models. In all experiments with cushion pipes which have been published so far everything is done to make the pipes as symmetrical as possible. Koschlakoff, as is known to the reader, studied the problem on membrane pipes and found that the controlling factor is the symmetry of tension. We first checked his observations with four pipes of angles of 180°, 150°, 120°, and 90° and, except for a few details, could verify his results.

The differences in tension were measured in terms of the interval between the pitches corresponding to the natural frequencies of the membranes. This pitch was found by blowing the pipe while covering one membrane carefully with a thin but rigid piece of cardboard which prevented it from moving but did not displace it, and thus left the conditions for the other membrane practically unchanged. This method was tested by plucking the edge of the membranes with a blunt nail. The tone obtained by this method was not perfectly but practically identical with the tone obtained by blowing the pipe with one membrane carefully covered. But the tone was so weak that it could only be heard at a distance of a few cm.; and, moreover, the plucking method took too much time, so the other method was generally used. Its disadvantage is that in all membrane pipes the pitch changes with the pressure of the blast. In our pipes, *e.g.*, at very low tensions, close to the slackened position of the membranes, the difference in pitch between the weakest and the strongest possible blast of the experimenter was about a fifth. At the tension used mostly in the observations this difference was not greater than about a second or, in extreme cases,

a distribution as was likely to invert or at least to compensate the asymmetries of position in case the walls were elastic. But characteristically this was only found with a dam at right angles to the canal, corresponding to the flat pipe of Müller and West, and not with oblique dams. But the devices available for measuring the pressures at different points round the slit did not fit our purposes exactly enough. This attempt was, therefore, abandoned.

a minor third; and with the medium strength of blowing which was used as constantly as possible the actual deviations were considerably smaller than a minor second. However, the measures are but approximate and possibly the expectations of the experimenter had sometimes a slightly modifying influence. The tension was controlled with screws which could move small sledge-like pieces at which one end of the membranes was fastened.

Here are the chief results. With an angle of 180° the symmetry could not be made perfect enough to obtain synchronous vibration, most probably, because our membranes (of dental rubber) were thinner than those of Oertel and his followers, and thinner membranes require a more delicate adjustment than our devices permitted. With 150° angle the greatest pitch difference with which synchronous vibration was observed was a minor third, for instance, the pitch of the common vibration being between f and f♯. The slightest increase of this difference made the vibration alternate and changed the common pitch into d. However, the range of synchronous vibration was so large only when the position of the membranes was perfectly symmetrical. Deviations from the symmetrical position, *e.g.*, one membrane being somewhat higher than the other, had as a consequence alternate vibration already occurring at a pitch difference of a half tone. In a pipe with a 120° angle, under favorable circumstances, the range of synchronous vibration is slightly wider than with 150°. But with greater differences of tension it is not replaced by alternate vibration. This mode occurs only occasionally here as a borderline phenomenon between synchronous vibration and "hoarseness"; that is, it occurs with such differences of tension at which the membranes, so to speak, do not know which of the two other modes of vibration they shall adopt and, therefore, fluctuate from one to another. The approach to this borderline was indicated by a slight lagging of one membrane.

Before we continue our account we have to describe the mode of vibration which was called "hoarseness" above and in which

a third and important mode of vibration of double vibrators was recently found.[55] Here either membrane makes a primary vibration of approximately its own natural period and a small secondary vibration of the period of the other membrane. While the primary vibrations, of course, have little relation to each other, the secondary vibration of each membrane is alternate to the primary vibration of the other membrane. The vibrations of both membranes are coupled and, therefore, the amplitudes increase and decrease continually so that when one membrane has the greatest amplitude the other one has the smallest and *vice versa.* The result is a double tone with all kinds of difference tones embedded into an awful rattling, the whole noise having all characteristics of a strong human hoarseness. The rough stroboscopic aspect is that of an alternate vibration with, however, a number of suspicious traits. The membrane to which the stroboscope is actually tuned looks sharper and usually appears to make larger vibrations; the other membranes look dim and their vibrations, apparently alternate to those of the clear membrane, seem to be of smaller amplitude. This aspect is, of course, misleading if the stroboscope is not tuned to either frequency and the two aspects carefully compared.

We continue describing the distribution of modes. With 90° alternate vibration was not found at all. The range of synchronous vibration seemed to be a little smaller than with 120°. At intervals of a whole tone the vibration already fluctuated between being synchronous and hoarse. But the synchronous mode of vibration still proved to have a greater stability than with 120° in another respect; when the difference was less than a whole tone, the membranes could be brought in positions as unsymmetrical as in Fig. 38 without disturbing the synchronous vibration. Koschlakoff '86 found a wider range of synchronous vibration with 90° than with 120°. After a careful comparison of our device with the one described by him (with which we became acquainted only after most of our experiments

[55] See footnote 24. Koschlakoff also observed hoarseness with too close contact of the two membranes; but, as we remember, this was the case only with flat membranes.

were made) we were led to believe that his device allowed for easier control of the factors of symmetry than did ours and that, therefore, his findings are more reliable at this point.

For studying these conditions on lips or cushions neither of the standard models of Ewald or Wethlo was available for the moment, so they were replaced simply by using membranes which were folded back. As Fig. 39 shows, the slit is formed by two lip-shaped bodies. It will be shown later that by this substitution no essential traits of the arrangement are changed. The folds mentioned were tested with all of the four angles between 180° and 90° just as the plain membranes had been tested. The chief result was that no alternate vibration occurred but only either synchronous vibration or hoarseness. The range of synchronous vibration was much wider than with plain membranes arranged in the same angles; under favorable circumstances (small angle and good symmetry) it stayed up to pitch differences of a fourth. This wide range is of great theoretical interest. If the human vocal cords really have a shape similar to this we can understand why the asymmetries that must be normally expected in it do not generally prevent the production of a clear tone. For in any other arrangement, but for the flat one which is least probable of all in the larynx, relatively small asymmetries would cause intolerable hoarseness. Some other traits which are strikingly similar to traits of the larynx are only found with folds and not with membranes as vibrators. If the difference of tension was considerable, *e.g.,* a major third, in terms of pitch, a moderate air blast caused a clear tone, its pitch being approximately the pitch of the tenser fold; but an exaggerated air blast at once changed the tone into hoarse polyphony which is a frequent observation in human voice production. All observers except Tonndorf agree that the narrow position of the vocal cords lasts much longer than the wide position in every period. This can be, and was, explained by the assumption that after bouncing together the vocal cords become flattened, and only after the flattening is overcome by their elasticity do they separate again. This explanation, however, holds only when

the vocal slit actually closes. The fold models showed another possibility which was mentioned before in I, 6; namely, a vibration of the shape that is represented in Fig. 31 and similar shapes. When this mode of vibration was first seen through the stroboscope it was doubtful whether it was not one more stroboscopic distortion. But when white dots were painted on the edges of the folds and the vibration was observed with the naked eye, the lines traced by the moving dots were exactly of the shape of the curves seen through the stroboscope. A number of other striking features of the fold vibrations proved to be due to stroboscopic distortion, as described in the last paragraphs of I, 7. It is easy to see how under such conditions the edges must appear in a narrow position most of the time when watched from above. It is also easy to see how with this mode of vibration curves of tracheal pressure can be produced like the ones found by Weiss '14 II. Attention is called to the strength of the second partial in Fig. 40. A few noteworthy traits which are peculiar to folded membranes in a flat position may be added. In this position the folds vibrated only when the distance between the edges was practically zero and when the tension of both membranes was much higher than usually. An enormous pressure of the air blast was required to start the vibration and that produced a very high tone. After it was started, however, the pressure could be lowered and with it the pitch also lowered a certain amount. When a certain normal pitch was reached this way a new increase of the pressure did not raise the pitch again, but lowered it a little. The whole mechanism seems to be rather complicated. The same sudden increase of the pressure sometimes made the tone rise and become slightly stronger, sometimes fall and become very much stronger.[56] In the first case the position of equilibrium seemed to be considerably displaced in the outward direction, while in the second case it was approximately identical with the state of rest. In the oblique positions none of these abnormalities were observed. The vibration could be

[56] Compare with this the results obtained by Ewald and Wethlo about the influence of air pressure on the pitch of their cushion-pipes. Ewald found rising pitch, Wethlo found falling pitch with increased pressure.

started with normal tension and pressure. Increased pressure raised the tone as in any other membrane pipe.

Summary of the above observations. Alternate vibration is the general mode of vibration in membranes at an angle of 180°, except with perfect symmetry. It is the mode of vibration of the greater differences in tension (with good symmetry beyond a minor third) in membranes at 150°. In membranes at an angle of 120° alternate vibration occurs only as a rare borderline phenomenon between synchronous vibration and hoarseness, which here is the mode of vibration of the greater differences in tension. In membranes at an angle of 90° no alternate vibration occurs; with greater asymmetry the hoarseness appears immediately. In lip-shaped folded membranes no alternate vibration occurs at any angle; there only occurs either synchronous vibration or hoarseness. The range of the synchronous vibration is wider than with oblique membranes.

The question of this section, whether asymmetries also cause alternate vibration in those models that are more similar to the larynx than the flat model, must therefore be answered negatively. The difference in shape between the flat model and the human larynx has such central functional consequences that this model can no longer be considered as a serviceable analogue of the larynx. On the other hand, the lip-shaped vibrators have a great number of traits which are peculiar to the human larynx and which in the case of simple membranes are neither found in the flat position nor at any other angle.

9. *The shape of the vocal cords.* The problem of the mode of vibration of the vocal cords is thus reduced to the problem of their exact shape and position during voice production. If it could be shown that the vocal cords during action really have the shape of lips, whether in oblique position or not, it would without direct observation be certain that their mode of vibration is synchronous.

But we have first to see whether our folded membranes are true representatives of lips as they are actually found in the larynx, for our folds are not connected with the walls of the pipe directly

as the real vocal cords probably are but by means of the flat parts of the membranes *BC and B'C'* in Fig. 30. Our argument is simply that with regard to their shape true lips differ from our folds in the same direction as the folds differ from simple membranes; that is, that the transformation which leads from membranes to fold-like bodies must be further continued in order to obtain true lips (Fig. 41). Since the function of the vibrators closely depends upon their formal character, eventual functional deviations of the lips from the folds consist with 100 per cent probability in a further increase of the features that distinguish the folds from the membranes, *e.g.,* perhaps still less disturbance of the synchronous vibration by unequal tension.

A few occasional observations on the models showed to which parts of the vocal cords attention had to be chiefly directed and hereby justified our approach again from another point of view. The original question was whether there was a connection between free vibration and alternate vibration, on the one hand, and between striking vibration and synchronous vibration on the other hand. The result was that these connections do not exist, for the alternate vibration of the flat membranes was not disturbed by contact nor was the synchronous vibration of the oblique membranes disturbed by separating them so far that no contact occurred. In this experiment it happened that the flat membranes were pushed too close together so that they leaned against one another with their edges, *i.e.,* a narrow strip of them along the edges came into an oblique position. The oblique position of this narrow region made the used pair of otherwise flat membranes vibrate synchronously like oblique membranes. And reversely, with the shape presented in Fig. 42a, the flat position of this narrow region made a pair of otherwise oblique membranes vibrate alternately like flat membranes. Thus, the narrow region of the vibrators right along the slit appeared to be the part that controls the mode of vibration. The controlling region was found to be still narrower. The vibrator Fig. 42c acts exactly like the vibrator Fig. 42b which is obtained if the (dotted) upper parts of Fig. 42c are taken away. The shape of the lower side,

therefore, seems to be of more importance than the shape of the top side.

In our investigation of the true shape of the functioning vocal cords our attention, then, will be concentrated on the region along and immediately below the glottal slit. What do we know about this part of the vocal cords? It is the only part of them that has not yet been investigated. Musehold and Scripture offer a definite opinion about it, but they obtained it only by reasoning from the shape of the top side, from general properties of the muscles which are covered by the cords, and from their observations of the action of the vocal cords. The models of Ewald and Wethlo were also designed with this indirect knowledge in mind.

The thing needed is a frontal view of the living and vibrating vocal cords. The natural means for this purpose is sagittal X-ray photography. But there is a difficulty. In ordinary pictures of this kind, with the tube in front of the subject and the plate behind his neck, the vertebrae will appear so predominantly that such soft structures as the cavity of the larynx will never be clear. The idea of evading this difficulty was derived from Aurelius Réthi. He took a number of very good sagittal pictures of the larynx during breathing and avoided the shadow of the vertebrae by putting the film into a specially adapted slide which was placed into the *sinus piriformis* and the upper part of the oesophagus, which is immediately behind the larynx at a distance from it not much greater than 2 mm. and at right angles to it just as if it were made for the purpose of holding plates for photographs of the larynx. If such pictures are taken with long exposure and low voltage, the shape of the cavity rather than the shape of the cartilages is seen. Especially up to a certain age where the cartilages are not yet ossified their transparency for X-rays is almost the same as that of the surrounding soft tissue, so that what appears on the picture can be nothing else than the shape of the cavity. And if ossification has already begun it is not hard to differentiate between the shape of the cartilage and the shape of the cavity because they do not grow in regular sym-

metrical form but are scattered here and there irregularly, very similarly to scrambled eggs, as can be seen in Fig. 44. In the adaptation of Réthi's device to our special purposes a trapezoid slide of polished sheet steel with a steel spring as handle was used (Fig. 43), height about 7.5 cm., lower width 2.5 cm., upper width 4.5 cm. After the first trial it was made narrower, at the top about 2.5 cm. wide and not more than 1.3 cm. at the bottom. The latter shape can be seen from the shape of the photographs Fig. 46. It was made this small in order to diminish the tension produced by it in the throat; but, according to the experiences of the subjects, a size lying in the middle between the two that were used by us with the side walls almost as oblique as in our larger slide would be most convenient of all, of course, only for normal adult male subjects. It was found that with both sizes in medium pitch the production of tones was perfectly normal with the slide sticking in the oesophagus. There was no difficulty in hitting an intended pitch. No greater effort was necessary than was normal for the subject. There was no displacement of the pitch, *e.g.,* like the displacement that is observed when the thyroid cartilage is pressed with a finger. Only the highest pitches could not be produced, obviously, because the arythenoid cartilages could not be tilted back as far as is necessary for the greatest tension of the cords. It seems to be safe to conclude from these observations that the slides in the oesophagus do not produce such pressures or tensions that the normal position and action of the vocal cords in the middle register is disturbed.

The pictures were taken with a 30 m.a.-Coolidge Radiator Tube. The clearest pictures were obtained with about 2½″ spark gap at 20 m.a. and 10–12 seconds exposure. The target-plate distance was about 24 in. It is perhaps desirable to present the total of minor precautionary measures which were made advisable in our experience.

(1) *Preparation of the slides.* When cutting the film into the proper shape cut a piece of clear thin celluloid with it. Mark the serial number of the films already cut with pin holes at the

upper end and use them in that order. Cover the emulsion with the piece of celluloid to prevent mucus from reaching the emulsion. Wrap the light proof paper so that the edges of the paper are at the back side. Be careful that the package is not longer than the slide and its edges do not scratch the subject's throat. After pushing the package into the slide as far down as possible fix it with strips of adhesive tape applied over the top at the two sides of the handle so it can not move during the introduction into the throat.

(2) *Preparation of the subject.* Local anæsthesia with a 10–20 per cent solution of cocaine which is painted on the walls of the soft gum, the throat, and the upper part of the œsophagus, in the customary manner, in portions applied at intervals of a few minutes each. Do not give too large portions at once, but rather a greater number of weak portions. Avoid painting the larynx so that the voice production is not disturbed. Do not give the last portion of the anæsthetic until the film is prepared and the X-ray machine is ready. The intervals between anæsthetizing are used for preparing the position on the X-ray table. The subject lies on his back, a small cushion under his neck, the head slightly bent back. The tube is not exactly vertical above his neck, as during phonation the vocal cords are tilted upward at the back end. The most favorable angle was found to be about 70–75°, that is, about 15–20° tilted toward the head. As one can readily calculate, this angle can be found by features of the photographs, since at the critical place only vertical lines appear clearly when the angle is wrong. The criterion for the proper angle, then, is that other lines are as sharp as vertical ones in the picture. With every subject the first introduction of the slide is made without the film. While the subjects sits before the fluoroscope it is found whether the slide is placed deeply enough and otherwise correctly. If it is all right a mark is made on the handle where it touches the teeth. Before introduction both sides of the slide are oiled with paraffine oil to make it glide better. During the introduction of the slide the subject holds his own tongue. He is told repeatedly to breath deeply, quietly,

and regularly in order to inhibit other reflexes. When the slide is in place he releases his tongue and holds the handle of the slide. He himself is better able than the physician to push it down the last few necessary millimeters without hurting himself. The farther the slide is down the less it hurts and the less it arouses reflexes. The subject then tries to produce a number of tones in order that he may become accustomed to the performance. After the observation with the fluoroscope the slide is taken out again and the subject is given a pause for recovery. In the meantime the slide is loaded. Then, a new moderate painting of cocaine is applied and the filled slide is introduced exactly as before. The subject places himself on the X-ray table; the position of the X-ray tube, the slide, and the body are carefully checked. A tuning fork is sounded (the fork used by us had a pitch e, 160 d.v.). The subject takes a deep breath and sings the tone of the tuning fork as exactly and as long as possible. When he starts the exposure is made. If the subject stops singing before the appointed time is over the X-ray machine is also stopped immediately.

Every film is developed at once in order to know and to eliminate eventual mistakes in the next exposure. Clips are attached only at the upper (wider) end of the film as important parts of the picture sometimes are very close to the edges, especially at the lower end. The whole performance is rather fatiguing for the subject. Réthi did not take more than two pictures at a sitting. We took four which was a maximum number because the subject became too nervous and tired. If pictures in the state of plain breathing were also to be taken they were taken after the pictures of the sounding larynx, because they required less control. It proved advisable not to take further pictures with the same subject until at least two weeks had elapsed. Among twelve pictures that were taken seven failed completely, two were fairly good (the 9th and 11th), and only three were satisfactory; namely, the 4th, 10th, and 12th. The method is still very crude. If we should try to repeat it, our first endeavor would be to make the walls of the larynx more opaque for the

X-rays. Lipiodol, argyrol, sodium iodide or bromide should be tried and, as far as we know, are being tried at present in West's laboratory.

The results of our attempts are contained in Figs. 44–47. Figs. 45 and 46 were taken while the tone e, 160 d.v., was produced, Figs. 44 and 47 during breathing. Both subjects were men at an age of twenty-seven to twenty-eight years. Figs. 44, 46, and 47 are from the same subject. Pictures of other pitches have not succeeded so far because of external technical reasons. The perfectly lip-shaped silhouettes in the lower part of the pictures represent the vocal cords in frontal view.

The following objections might be made against the opinion that these structures are really the vocal cords: (1) That the lip-shaped silhouettes represent cartilages or ossified parts thereof. This argument has already been refuted; we refer to the fifth paragraph of this section and to the aspect of real ossifications which appear besides the lips in Fig. 44. Besides, what cartilages could the lips possibly represent? If they are parts of the cricoid, it can not be understood why there is such a strong fissure in the middle. Moreover, the cricoid cartilage can only be moved or tilted in the vertical direction, otherwise they must have exactly the same position and shape in singing and in breathing. This is not true for our lips, according to Figs. 46 and 47. As to the arythenoids, the fissure would not be in contradiction to their position, neither would the change in the width of the fissure. But in Figs. 48a and 48b we give their shape as it must be expected during breathing and during singing in a frontal view. The reader may compare this with Figs. 46 and 47 and draw his own conclusion. (2) That the vocal cords during the production of the tone mentioned are not so far from the hyoid bone as the lips in our pictures. We answer that in our pictures the hyoid bone is much more distant than usual from the thyroid cartilage because the head was bent back when the pictures were taken. When the head is bent back the hyoid bone moves upward with the chin, though, of course, not so much as the chin. It is perfectly safe to assume that the lip-shaped structures in our

photographs are the vocal cords, the dimmer formations above them the false cords, and the brighter triangular part at the top between the upper horns of the thyroid and below the hyoid is the entrance into the *sinus piriformis* or upper œsophagus.

Our pictures are approximately natural size. They show remarkable differences in the general constitution of the two subjects. Yet, there is no difference in the essential traits of their vocal cords. Their shape is that of relatively thick lips. They are attached directly to the side walls of the pipe, in a strikingly oblique position, their lower surfaces forming a wedge-shaped air conduction which grows more and more acute in the immediate neighborhood of the narrowest spot of the glottal slit. They have all the properties of those models in which, as we found, alternate vibrations can not occur. We conclude that the mode of vibration of the human vocal cords is normally synchronous.

Of course, the shape which we found will not be found in all pitches. But, on the one hand, what is true for intermediate pitches is all the more true for lower pitches. And, on the other hand, in the higher pitches even if there the vocal cords, or, as we shall call them in the future, the voice-lips, should be much thinner than in our pictures, their position can hardly leave the oblique position in which we found them. On the contrary, there are symptoms of the fact that in the higher pitches the voice-lips become more and more oblique. It is known that in the highest pitches no tone can be produced during inspiration. In intermediate pitches this is easily possible. Now in the models with flat or only slightly oblique membranes, or cushions of a corresponding position, as in Ewald's model, the direction of the air blast is of no importance, the pipes work in both directions. This is not the case with oblique membranes or cushions, as in Wethlo's model. Beyond a certain pitch inspiration no longer starts a tone but simply closes the slit and, thus, stops itself. The chance, therefore, is that in higher pitches the position of the voice-lips is still more oblique than in medium pitches and that there is no room for alternate vibration at any pitch.

Our findings are in full agreement with the results obtained

by the modern German laryngologists and by Scripture, with direct observation of the larynx from above, and show that the models designed by Ewald and Wethlo are approximately correct, Ewald's more for lower pitches, Wethlo's more for higher pitches. In Fig. 45 we happen to have an example of the actual asymmetries of the larynx which were found also by Hegener '21 in his stereoscopic laryngoscopy. The voice produced by this larynx is not just an artistic voice, but it is perfectly normal. Our findings differed from what was expected from the authorities mentioned only in that there is a relatively sharp angle between the lower surface of the voice-lips and the side walls of the larynx. None of their designs contains this angle.

In this angle we find one more strong argument against the other 1926 theory of voice production, the theory of Weleminsky. In his assumption the movements of the voice-lips are only secondary movements accompanying the movements of the real vibrator which is the elastic membrane stretched between the edges of the voice-lips and the cricoid cartilage. The inside of the larynx, then, should obviously form a triangle and during vibration the dimmest part should, of course, be halfway between the basis and the top of the triangle. But our pictures show clearly that the membrane is not stretched the way he assumes, but covers tightly the muscular and cartilaginous parts of the inner larynx; and halfway between the edges of the voice-lips and the lower end of the larynx the contour is not dimmer than at any other place.

10. *The perceived tone.* The assumption that because of alternate vibration the tone produced by the voice-lips originally has the double frequency of their own vibration would, of course, make difficulties in accounting for the perception of the fundamental instead of the second partial in singing, at least after it has been verified by Tonndorf '26 that the perceived pitch and the voice-lips themselves have the same fundamental frequency. Though this problem no longer exists for the larynx since it has been found that the voice-lips move synchronously, it shall be briefly treated in terms of the flat membrane pipe since it

involves basic traits of voice production and perception. West (pp. 260–270) offers two confused explanations. The first explanation which he unfortunately prefers is as follows: "The tones produced by the two vocal bands" . . . may be "slightly different in quality" (p. 266). That means two tones are produced simultaneously, each by one of the two bands, and "the resonator has ability to respond only to one or the other of the two vibrating bands" (p. 260). This description is, from the standpoint of physics, as impossible as the description of the action of the two analogues, the tuning fork and the mandolin (p. 261 f). In neither case does the resonator have the effect of making one-half of a two-sided process disappear and of intensifying the other half of it; but always it compounds the action of the two sides in a suitable way. We shall only consider the double membrane pipe in this regard. West supports his opinion by the following observation (p. 260). When the opening of a narrow Y-tube which is connected with the two ears of the observer is moved across the cap of his model, at right angles to the slit, the ordinary tone is heard when the opening is placed above the two bands; but when it crosses the slit the pitch is at once perceived to be an octave higher. His interpretation of this phenomenon is that "the tube . . . being held immediately over the bands *caught very little of the resonance;* but the ears unaided by such a device received the air waves that the resonance chamber is suited to amplify."[57] There is apparently some confusion here about the action of a resonator. The magneto-phonoscopes have shown that any point outside of the walls of the box, or resonator, make only double vibrations (p. 252). The only point where the interior of the box is in communication with the surroundings is the slit which furnished double vibrations in the observation just described. It is true that West thinks the octave does not come from the interior but from the two edges alternately; but, according to himself, "this phenomenon will not be apparent if the opening of the tube is too large." In other words, it will not be apparent when the

[57] Italics are the writer's.

opening of the ear tube is wider than the slit and thus catches more than what comes exactly vertically out of the slit. That means that any directly observable action of the box, or resonator, has a double frequency. And still this box by a mystical power shall bring it about that at a distance the fundamental and not the octave is heard. It was possible to throw light into this confusing situation by comparing the finer qualitative and spatial attributes of the tones produced by a flat pipe and by the standard oblique pipe of 90° angle.

The tones characteristic of these two pipes are not due to resonance. If the vibrations of both tubes are tuned to the same speed of the stroboscope the tone of the oblique pipe is identical with the siren tone of the stroboscope; the tone of the flat pipe is an octave higher in pitch with a perceivable ingredient of the lower octave. Rather, from a greater distance almost the only partial heard is the second, but when the observer approaches the pipe the fundamental grows stronger, never becoming really prevalent. The tone of the flat pipe is more smooth and clear. The blending of the two octaves is not far from being complete. The sound of the oblique pipe is more metallic; it seems to have a buzz in it. It is full of high overtones, but the first overtone can not be more readily distinguished than the others. Since in tube resonators the length is the main controlling factor this comparison was made with two equally long tubes. The resonance of the tubes was then varied through two octaves. If the characteristic features of the tones were due to resonance, typical effects of this change should be expected. In the flat pipe the octave should disappear or at least be much weakened and the tone should become similar to that of the oblique pipe except for the buzz, if the resonance of the tube was that of the fundamental. On the other hand, the fundamental should disappear or be much weakened if the resonance of the tube was that of the octave. Nothing like that happened. If there were changes in the ratio of the intensities of the two octaves, these changes must have been subliminal for the characteristic sounds of the two pipes remained essentially unchanged

throughout the range of resonance. The changes that were observed concerned only the vowel quality of the tones.

The second partial is perceived not because the opposite vibrations of the two membranes are added together. As we showed above in I, 2, it is known since 1825 that the tones produced directly by the two membranes are practically zero. And if they were worth considering their combination would never produce an octave vibration, but they would mutually compensate one another. This rule does not allow exceptions. The tones perceived are a direct function of the slit; and the second partial is perceived because alternate vibration makes two openings of the slit for each full period.

The fundamental is heard in the flat pipe because the two subsequent openings that correspond to a full period are never alike.[58] Here we have West's second, and sound, idea about the perception of the fundamental (I, 6). Even when the two membranes are perfectly symmetrical so that the two openings of one period are perfectly equal, the fundamental must still be perceived, except under very specialized conditions, for the position of the membranes in two subsequent openings is inverted and the two pertaining puffs of air are in opposite directions. So they must behave as if coming from two different distant sources alternately. They must reach the ear in pairs of unequal members, except in the case where the two imaginary sources have exactly the same distance from the ear of the observer. This is substantiated by the following observation. When the flat pipe is held immediately in front of an ear, the fundamental is strongest when the slit is parallel to the wall of the head; but it completely disappears when the pipe is turned 90° so that the slit lies in the connection line of the two ears. The reason is that in the first case one puff is directed towards the ear, the other away from it; in the second case both are equivalent. Another substantiation is contained in the disappearing of the fundamental at greater distances. This is but an effect of parallax. The angular difference of the two subsequent puffs decreases

[58] By this statement the chief argument of J. Müller against W. Weber's theory of reed pipes (See Müller '40, p. 176) is invalidated.

with the distance; the imaginary sources of the two corresponding puffs gradually become identical; and so the two relative waves become more and more equal. A third instance supposes that if the center of pressure changes with every puff, this must have an influence on objects sensitive to such changes when placed immediately above the pipe. This was found to be true. When a postal card was held immediately over the slit of the flat pipe exactly parallel to the slit, the card vibrated violently and strong resonance of the fundamental was obtained. This was not obtained in any other position of the card, whether turned or shifted. As a last instance we take West's observation with the narrow ear tube. The intensities of the fundamental and the octave are exactly distributed as is shown in Fig. 49, the abscissa representing the width of the top surface, the ordinate the loudness, the broken line the fundamental, the dotted line the octave. The effect is explained by the fact that puffs, which are not exactly separate, are produced alternately in two different directions, but a continuous jet of air is coming out which is constantly tilting from one side to the other and is weakest, but does not completely disappear, while passing through the vertical position. Attention is called to the stroboscopic pictures of smoke jets obtained from the flat and the oblique pipes in Fig. 50. During every period the jet passes two times through the vertical position, and therefore twice enters the small ear tube, thus produring the octave. If the tube is too wide the jet enters the tube continuously and the conditions are as in the open air. The perceived pitch, then, depends only on the ordinary changes of pressure and, since these were in pairs of unequal members in West's experiment (see I, 6), the fundamental necessarily was perceived.

How would the above results apply to the larynx if the vibration of the voice-lips were alternate? First, the laws of resonance are exactly the same as in the models so the perception of the fundamental could not be an effect of resonance. Second, the perception of the fundamental could not be explained by the spatial attributes of alternate vibration, because the long air

conduction above the vocal lips would reduce to zero all effects of parallax. The only possible explanation would be by structural asymmetries of the voice-lips themselves, as described in I, 6, of such a nature that the two openings of each period were of different size and duration. But even then no resonator would be capable of suppressing the prevalence of the second partial, neither in male nor in female voices, neither in low nor in high pitches, according to the experiences of Weiss '14-I. So for all those cases in which no prevalent second partial is found, as in high pitches and in female voices, the impossible assumption of perfect symmetry of the larynx would be necessary. With the synchronous mode of vibration which is required by the real shape of the voice lips, each full vibration opens the slit only once and the air vibration has the same frequency as the lips. This is not changed by moderate asymmetries. Eventual 8-shaped vibrations could only produce pairs of very unequal humps so that the fundamental would never disappear. Thus, there is no difficulty in accounting for the perception of the fundamental pitch in the human voice.

But again the question arises: What is the cause of the double vibration of the "Adam's apple"? To answer this question will be the task of the next section.

11. *The cause of the double vibrations.* The phenomenon of the double frequency which was easy to solve for the rubber model (I, 6, 10) is not so simple and general in the human larynx. West found that the double vibrations are also obtained in observations of the voice and not only of the "Adam's apple." But in the voice they are found only in adult men, not in women and children, and besides this only at lower pitches, *e.g.,* at 128 d.v., and not at higher pitches, *e.g.,* at 256 d.v. Unfortunately he does not present any laryngeal tracings, except from adult men and at the frequency of 128 d.v., so that we do not know whether the occurrence of the double vibrations in the larynx is as limited as that of the double vibrations in the voice. But it seems to be possible to deal with the problem without this knowledge.

We first briefly reconsider the possible influence of tension.

In I, 6, we found that by the argument based upon his model experiment by West (p. 252) this influence was not excluded for the larynx, but that his tracings from the larynx contain an argument against this assumption. We found that the double humps should be of equal size and shape if caused by tension acted upon the "Adam's apple" at the point of attachment of the voice-lips, which is in contradiction to the actual tracings. But, even if other tracings would be obtained in which the double humps are exactly equal, there would be other criteria about the factor of tension. By its nature it can hardly have an influence upon the pressure curve of the air column. That means that, if curves were taken simultaneously from the "Adam's apple" and from the mouth, and the double curve were the rule in the laryngeal curves but not in the mouth curves—if, for instance, the double curve were also found on the "Adam's apple" of women or children though not in their voice, then it would be safe to assume that the double curve is due to the tension. *Rousselot* took such pictures for other purposes.[59] A cursory inspection of these curves shows that five times the second partial is stronger in the larynx, five times it is weaker in the larynx, three times there is no noticeable difference. That means that in the vibrations of the "Adam's apple" the tension can not be the controlling factor. It may be mentioned that in the violin, where this factor was first investigated, its influence was also found to be unexpectedly limited. It may further be mentioned that, according to our last argument, the double vibration in men's larynxes, if due to tension, does not account for the double vibrations in men's voices; and that for the double voice curves obtained by West, it would, therefore, be necessary in any case to seek for another cause.

When West observed men's voices with the manometric flame, he found that the curves "change with pitch and with change from one vowel to another, but retain their di-maximal (two-humped) shape." In the tracings of Rousselot that were mentioned above, and in some others taken by ourselves, the di-maximal shape occurs only occasionally. Now the Koenig

[59] Rousselot, Vol. II, Fig. 509.

manometric capsule that was used by West in his experiments is not a very reliable device, especially in such fine observations. But still it is very probable that his observation is perfectly correct though not so generally valid as he seems to think; namely, under the assumption that he observed only "open" tones. Musehold '13 mentions studies by *Piehlke* and Gutzman [60] on the nature of open and closed tones in singing. The open tones are more nasal, more similar to a trumpet tone, with the typical buzzing ingredient. The closed tones have a much more damped character. Piehlke found, according to Musehold, that in the closed tones the fundamental is especially pure while in the open tones, besides very high overtones, the second partial is prominent. Now, the normal tone of a man is the open tone, and it would not be strange if West observed only such tones, and thus never obtained other than di-maximal curves. But what, then, is the cause of the difference between the open and the closed tones? All experiences since Musehold '97 point to the explanation that in the open tone the voice-lips vibrate with contact, and in the closed tones without or at most with very light touching. When our fold pipes vibrated with strong contact so that they had a hum or buzz in their tone which was similar to the hum in the open male voice, we found that they made 8-shaped vibrations. If in the open tones of the human voice the form of the vibration were of the same kind, this would account for the strong second partial as well as for the buzzing.

There is one more important feature of the double vibration of male voices described by West which is not yet accounted for by the distinction between open and closed tones; namely, that there is, or "seems to be, a point of 'maximum resonance' at which these double humps appear to be of equal size and shape" (p. 258). And the most striking thing is that the pitch with which the strongest octave is found is just within the narrower range of a man's speaking voice, at frequencies of about 128 d.v. If the octave itself is intoned, not the next higher octave but the intoned pitch itself (256 d.v.) appears intensified. The latter

[60] Besides Piehlke, see Gutzman '11.

statement is true for men's as well as for women's voices, and, since women's normal speaking range is of about the pitch of 256 d.v., no abnormally strong second partials appear in their voice curves. This resonance maximum at about 256 d.v. is not disturbed by any change in size and shape of the nasal and buccal resonators and persists apparently, also, during the attempt at producing closed tones. This is the impression obtained from a number of tracings that were made simultaneously from the point of the thyroid cartilage and the mouth opening, as a check of West's statements. The apparatus used had a resonance maximum of 200 d.v. Nevertheless, the maximum intensification appeared not at 200 but at about 250 d.v. At about 125 d.v. we found a strong tendency towards double vibrations, but it was not so absolutely dominating as West described it (Fig. 51).

Therefore, there must be a resonator which responds normally to about 250 d.v. and perhaps also to 125 d.v., but not to the next higher octave; and this resonator can not be the nasal and buccal cavities. For pipes only cavities come into account as resonators. Since in our case it can not be the cavity above the larynx that resonates to these pitches, there is apparently only one solution; namely, that it is the cavity below the larynx, consisting of the inside of the larynx, the trachea, and the wider parts of the bronchial tree. *Giesswein* independently came to the conclusion that the bronchial tree, taken as one unit from the voice-lips downward, must have a natural frequency of about 128 and 256 d.v. He devoted careful studies to the problem of resonance of tubes similar to this cavity in their size, their shape, the qualities of their own material, and the qualities of the material of the surroundings in which they are embedded. His result is that a tube resonator of the same length as the bronchial tree, no matter whether it is cylindric or tree shaped, if it has partially membranous walls like the bronchi and is also embedded in a foamy mass and under similar conditions of temperature, has a natural frequency of about 128 d.v. and resonates to this range of pitch and also to the next octave, but not to the third octave. Giesswein determined the bronchial resonance in human indi-

viduals by means of the maxima of vibration of certain parts of the chest which can easily be found. Since the pitch of the resonance depends essentially on the length and not on the width of the resonator, the resonance in women can not differ very much from the resonance in men. Giesswein found it to be, *e.g.*, around a^{-1} and a^0 in a tall man with a bass voice; in baritones and tenors it was of course higher, sometimes as high as e^0 and e^1. In women he found the chief response at about 256 d.v., that is, between a^0 and e^1, and a weaker one an octave lower if they were able to go down to such a low pitch, *e.g.*, at d^1 and d^0. From this agreement between his model resonators and the human chest vibrations Giesswein concludes that with high probability the chest vibrations in the normal pitch range of speech are due to resonance of the bronchial tree; and, moreover, that the ease with which it is possible to talk in this pitch for any length of time, but not in other pitches, is due to the assistance of the bronchial resonance to the vibrations of the voice-lips.

Does this help to clear up our own problem? It seems to us that it does. When a man talks in his natural pitch of c^0 this tone as well as its octave are amplified by his bronchial resonance, and besides a strong fundamental a strong second partial must appear in his voice curve. When he talks at an octave higher the bronchi respond only with their second partial and no octave resonance appears. Since women's voice-lips are much smaller than men's but their bronchial tree only slightly shorter than in men, their natural talking pitch is adjusted to the second partial of the bronchial resonator; and, since it does not respond with any higher partial, only the fundamental pitch of their voice is amplified. This is exactly what was to be explained. It may be added that for the octave response of the resonator in men the octave need not be contained in the vibration of the voice-lips. A resonator can, under certain conditions, respond with any of its natural overtones if stimulated with a tone of its fundamental pitch.[61]

All the sex differences and the peculiarities of the male voice

[61] Raman, 1912 I and II, 1915.

development can be explained on this basis of bronchial resonance without relying on the alternate mode of vibration of the voice-lips, which was found to be physically impossible. But the treatment of these facts lies beyond the scope of this study. Only a few experimental questions may be put by which it seems to be possible to verify the presented theory of the double resonance. Since the bronchi are considerably larger in deepest inspiration than in extreme expiration, in tones produced under these two conditions the pitch of maximal resonance should be different. Giesswein offers a few instances for that, but for our different purpose new exact observations seem to be necessary. A second question can be developed from West's observations on the French horn. If these observations are right, the lower resonating cavity in blowing this instrument should be the bronchial tree plus the mouth cavity if while blowing the horn the vocal slit is wide open, or only the mouth cavity from the larynx upward if the vocal slit is in a very narrow position, because the mouth-lips are the vibrators.[62] Most probably it will be still easier to answer this question experimentally than the first one.

III. *Summary*

The first existing frontal view of the vocal cords during voice production is presented. It is shown that during action the vocal cords have not the shape of bands but of lips, and it is suggested, therefore, that the term "voice-lips" be substituted.

[62] It may be mentioned that from this point of view new light is thrown on the difference between the behavior of a French horn and of a saxophone, as found by West, pp. 259, 260. Not because "the saxophone has but one vibrating body, while the French horn has two, the French horn resembles the human voice in that it has similar pitches of maximum resonance which sometimes enforce the octave; but because the vibrators of the French horn (*i.e.*, the mouth lips) open forward exactly as the human voice-lips do, while the vibrator of the saxophone opens backward. The action of reeds which open backward is entirely dependent on the resonance tube, that is, the body of the instrument, the resonance of which changes stepwise in the saxophone by its valves when the scale is played; whereas reeds which open forward do not by far so much depend on the resonance tube and therefore leave room for the influence of the "air chamber", *i.e.*, in our case, the mouth cavity, etc., of the player, the resonance of which remains practically constant and thus produces the effects observed by West.

From comparisons of the view of the larynx with acting models it was concluded:

(1) That alternate vibrations of the voice-lips are physically impossible, but that there are no mechanical objections against the results of the laryngoscopic observation of the last three decades, according to which their vibrations are in phase, and this is true also for the mouth-lips in trumpet-blowing.

(2) That the action of the voice-lips is a cushion action, as Ewald assumed in 1897.

(3) That the double curves obtained at the point of the "Adam's apple" very likely are caused to some extent by the special mechanism of "open tones," and also by the second partial of bronchial resonance, the maxima of which lie at about 128 and 256 d.v.

(4) That no adjustment of the pharynx can change the pitch of the voice to the next higher or the next lower octave.

(5) That the sex differences of the voice and the normal and abnormal phenomena of the boy's puberal change can be reduced to the different relations between the frequency of the vibrating voice-lips and the bronchial resonance.

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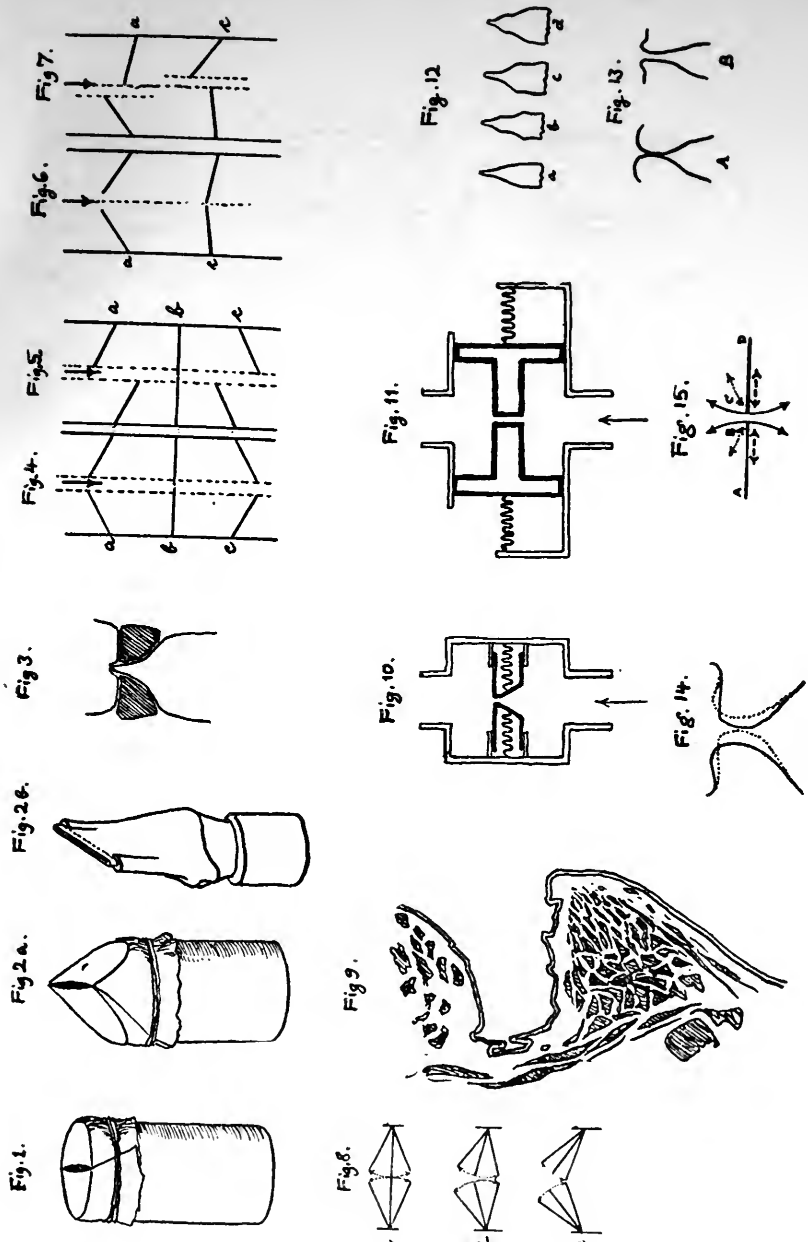
Figures and Photographs



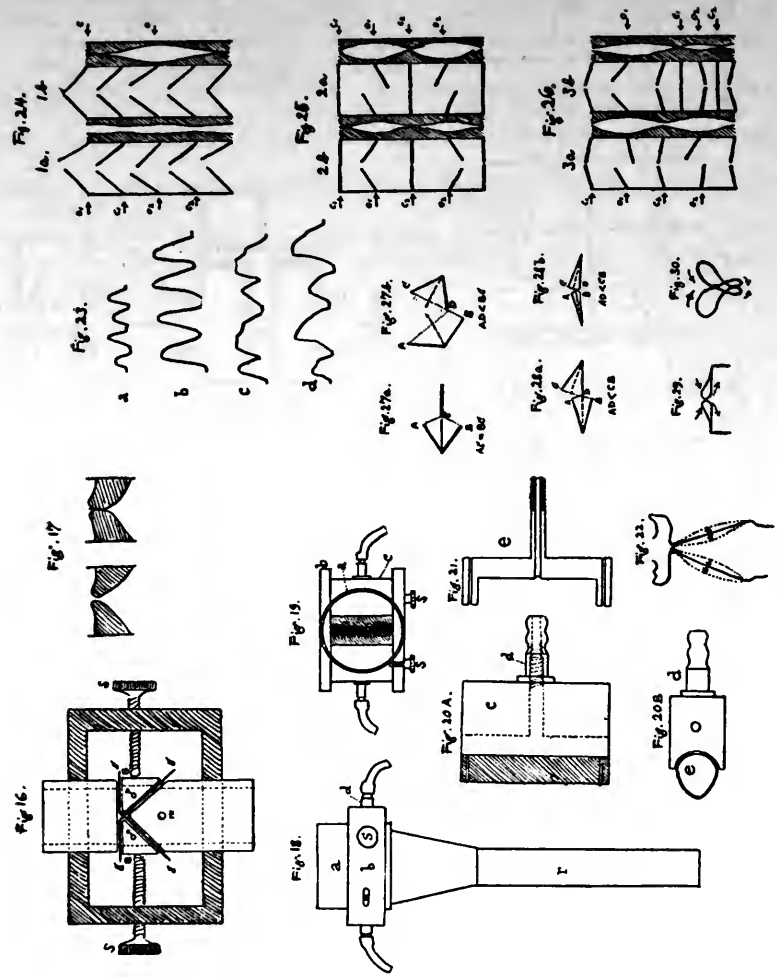


Fig. 16.
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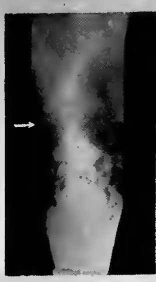
Fig. 44



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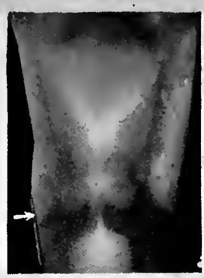

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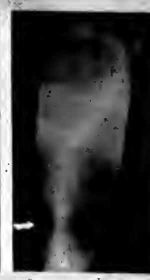

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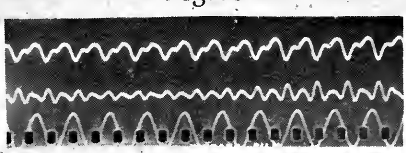

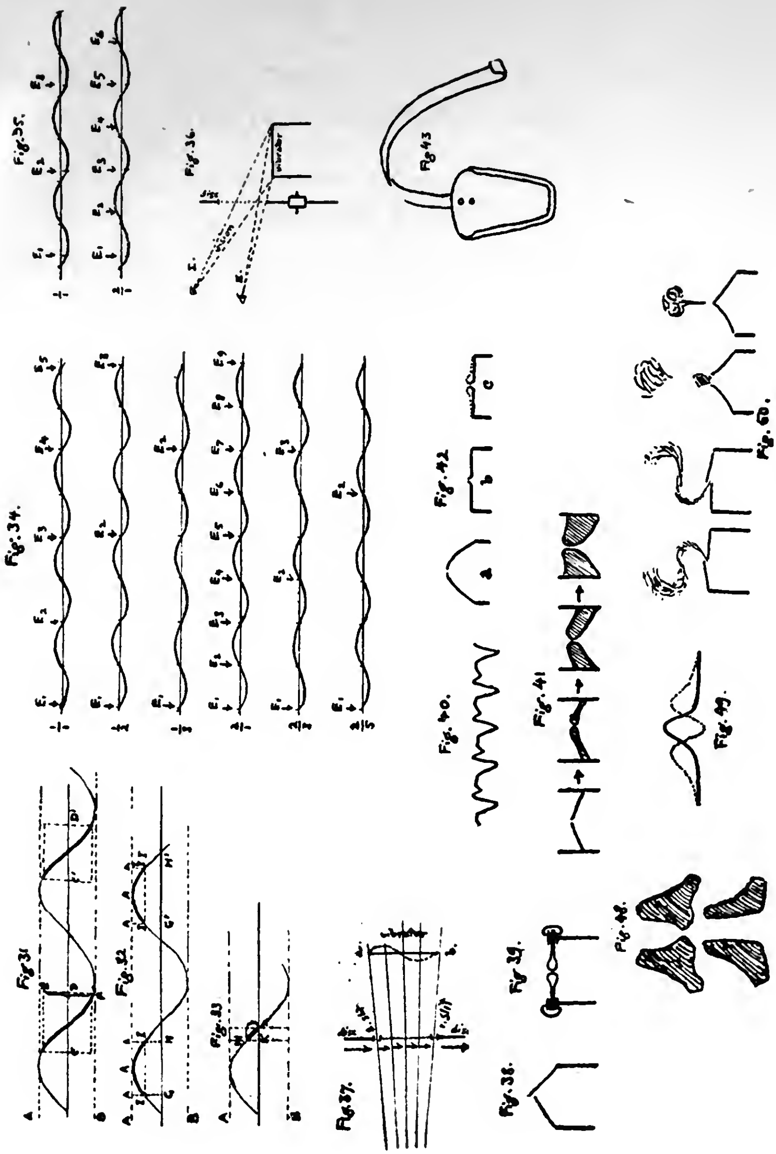
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Fig. 49.
Fig. 50.

A VACUUM TUBE TECHNIQUE FOR THE FREQUENCY RANGE OF PHASE EFFECT [1]

by

Erving N. Peterson

Introduction; apparatus and technique; discussion of results; theoretical significance of results; conclusions.

Introduction. Only a few years ago the existence of an auditory space was seriously questioned; today, in one form or another, it is generally admitted. The psychologists of the *Gestalt* school are occupied with the attempt to prove its configurational nature. Other psychologists are interested in auditory space from the standpoint of a complete mapping out of the perceptual function, and also for the light it throws on theories of audition. In military and naval circles it is studied for its significance in detecting tunneling and mining operations, for its use in detecting submarines and airplanes, and as a means of circumventing the effect of smoke screens. It interests radio engineers who wish to make reception faithful to the broadcasting program.

One of the most interesting problems in the field of sound localization is the question of the existence of a phase effect, the nature of the effect, and the approximate range of pitch over which it may be perceived. It has been the purpose of this study to develop a technique sufficiently comprehensive to deal with this problem in an effective manner. This paper aims to report the progress already made in realizing this technique and to give tentative conclusions on some of the questions raised.

Apparatus and technique. In the investigation of the frequency range of phase effect there are at least three items to be controlled: pitch, phase, and intensity. There should be a very

[1] This work was done under the direction of Dr. Christian A. Ruckmick.

wide range of frequencies, a wide range of phase adjustments, and a means of balancing the intensities. These three factors must permit of independent regulation of one factor without disturbing the other two factors, so that, for example, one may regulate the intensity without affecting the phase. Since the electron tube may be caused to produce an alternating current with a range conservatively estimated as between one and one million cycles per second, this method was adopted as the basis of the pitch technique. Since the phase relations were known to vary with the amount of self-inductance in the circuit, the variometer principle was first adopted as the best approach to phase control. It was also known that the intensity of a sound might be varied without interfering with the pitch or phase relations by the regulation of the filament current in an amplifier circuit. The amplifier or telephone repeater method was therefore adopted as the basis of intensity control. For reasons too involved to explain here it was decided to use condensers for obtaining phase changes. The heterodyning system was chosen for generating the audio-frequency current, since without much apparatus it permitted the utmost latitude in frequency. It was also decided that the use of a single source would give a better control of pitch than having two independent sources.

A few of the fundamental facts concerning the production of phase changes in oscillating circuits may now be reviewed. In a circuit containing resistance only, the current and the voltage are in phase. When the circuit contains inductance only, the current lags behind the voltage by 90 degrees. When the circuit contains capacity only, the current leads the voltage by 90 degrees. The above cases are of course mainly hypothetical. For example it would hardly be possible for a circuit to have inductance without some resistance also. Nevertheless the difficulty is theoretical and not practical since the conditions may be very closely approximated. When a circuit contains both inductive and capacitative reactance, their algebraic sum divided by the resistance will give the corresponding phase angle. The formula for capacitative reactance is

$$X_C = \frac{1}{\omega C} \qquad (1)$$

where C is the capacity in farads and ω is the frequency times 2π. The formula for inductive reactance is

$$X_L = L\omega \qquad (2)$$

Where L is the inductance in henrys and ω has the same meaning as in (1). For analytical purposes the phasing circuit may be stripped down to its barest

essentials as shown in Fig. I. L_1 and L_2 of Fig. I are the primaries of air core transformers, consisting of shielded honeycomb coils. C represents any desired capacity one might wish to include in that branch of the circuit. Notice that circuit θ was always a lagging circuit, and the circuit ϕ was always taken care of by convenient switching arrangements. In terms of this diagram (Fig. I) the laws of phase relationship may be stated in the following formulas:

$$\tan \theta = \frac{\omega L}{R} \qquad (3)$$

$$\tan \phi = \frac{1}{\dfrac{\omega C - \omega L}{R}} \qquad (4)$$

where R is the resistance of the corresponding branch. Using these formulae, the phasing apparatus was calibrated in discrete steps of 45°, 90°, and 135°, with a frequency range of 20 to 16,000 cycles. Although 10 cycles is usually considered below the limit of audibility, this was also calibrated for the 135° setting. The other settings were excluded on account of the number of condensers required. For frequencies under 512 cycles the honeycomb coils were excluded from the circuit and the 50 henry primaries of the transformers served as the inductances instead.

The pitch apparatus also deserves some explanation from an analytical standpoint. Fundamentally the circuit is that shown in Fig. II. This circuit will

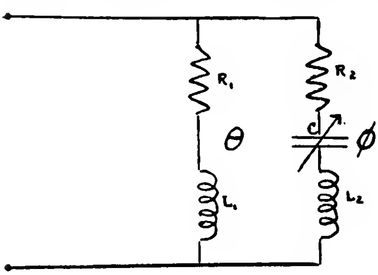

Fig. I

R_1—R_2—Resistances
L_1—L_2—Inductances
C—Capacity
Note that θ is always a lagging circuit since it contains no capacity.

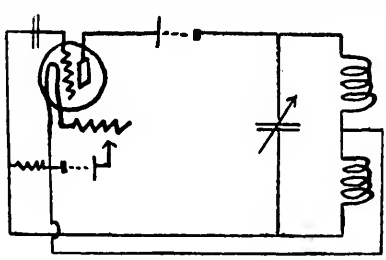

Fig. II
Fundamental oscillating circuit

maintain oscillations at a steady frequency as long as the proper amount of filament and plate current is supplied. While its action is somewhat analagous to the ordinary door bell and the electrically maintained tuning fork, the fundamental electrical principles involved are those of resonance and regeneration. Since it was decided to use the heterodyne circuit as the generator or master oscillator, it was necessary to use two of the circuits described above (Fig. II), and the constants of these circuits were finally fixed so that a basic

frequency of 50,000 cycles was obtained. Thus, for example if one tube oscillated at 50,000 cycles and the other at 49,990 the resulting audible beat-note resulting from the interference of the two circuits would be ten cycles. It is evident that two radio frequency currents when interfering produce an effect analagous to beats in acoustics. The interference modulates the waves giving them an audio-frequency outline or envelope. These two circuits now

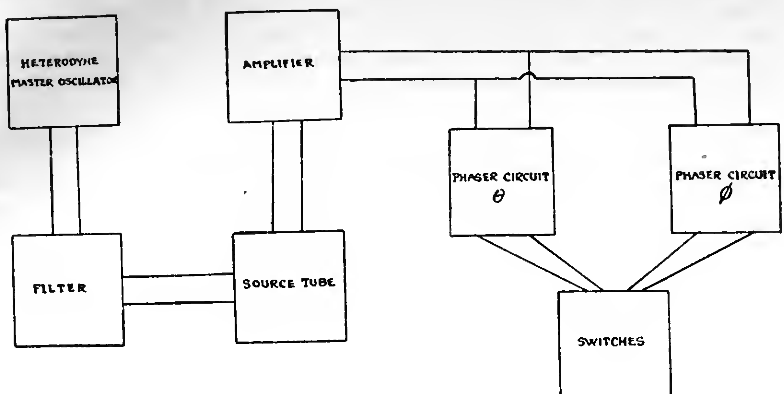

Fig. III

SCHEMATIC DIAGRAM OF APPARATUS

act as a master oscillator controlling the oscillations in a third tube which might be called a detector, or source tube. It is this controlled tube which supplies the forced oscillations for the parallel branches of the phasing circuit shown in Fig. I. The complete apparatus is now shown schematically in Fig. III. In Fig. IV is presented a symbolic diagram of the complete circuit. Fig. V is a photograph of the apparatus with the oscillator shields removed. The zero switch shown in Fig. IV affords an automatic theoretical phase control when a zero setting is desired since this switch makes it possible to supply both ears from one vacuum tube.

Pitch adjustment was made by comparisons with tuning forks for the lower frequencies and with Koenig bars for the upper frequencies. The forks were all accurately calibrated and the Koenig bar frequency was always divided by two to give the correct frequency in cycles. The writer's sense of pitch discrimination and intensity discriminations, as measured by the Seashore tests, are both well above the 90th percentile for adults, leaving little opportunity for subjective error in this direction.

As regards the electrical apparatus, it was carefully selected for the purpose in hand. Thordarson 2:1 matched audio-transformers were used in the phaser amplifying circuits. The air core audio transformers were measured under load conditions and an important correction of their inductance ratings made. Their resistance was measured by an ammeter and voltmeter and checked on a direct reading ohm meter. Among the varities of fixed condensers used were the Kellogg, Freshman, Dubilier, Western Electric and Sangamo. The Sangamo has an accurate calibration and was the condenser used chiefly at 8,192 cycles. Several of the Dubilier condensers were measured by the resonance method

and their actual capacity was remarkably close to their rated capacity. The supply of condensers was large and it was always convenient to check them by balancing one against another. The vernier condenser was of the straight line capacity type and its calibration curve was known. While capacity and inductance change with frequency, the changes at audible frequencies are slight and may be neglected. There never was any question which circuit was leading and any mistakes which crept in could only have been relative and of minor consequences, since the more important matters were checked up.

The receivers were the Brandes Superior matched tone type. Four receivers were on hand and they were frequently interchanged. At no frequency did they exhibit harmful resonance effects.

Detailed summaries of previous results on the range of phase effect are to be found in Stewart [2] and Banister.[3] The upper limit of phase effect has been variously placed from 512 cycles up to 2,000 cycles. Stewart places the upper limit at 1,260 cycles and investigated frequencies as high as 4,000 cycles. Banister places the frequency limit at 1,700 cycles. No investigation has been carried on in this field with frequencies lower than 100 cycles. The investigations which have been carried on by military and naval authorities in the United States and in other countries have been guarded as military secrets and have not been available for use in civilian research.

Using the apparatus described in the preceding pages, frequencies ranging from 10 cycles to 16,384 cycles have been explored for phase effect. After a considerable amount of preliminary experimenting quantitative measurements were made to see if there was a regression toward zero of the observed phase angle as one ascended from the lower to the higher frequencies. The observer was seated about a meter from the switch boards with his back turned to the apparatus. After the *O* was supplied with a chart to aid in visualizing the deflection, the intensities of the two phones and the pitch were adjusted. Settings of 45°, 90°, and 135° were then given in chance order, both to the left and to the right. The *O* had no possible cue as to whether the next setting would be to the left or to the right. Neither could

[2] Stewart, G. W. The intensity logarithmic law and the difference of phase effect in binaural audition. *Psychol. Rev. Mon. Supp.*, 31, 1922, No. 1 (University of Iowa Studies in Psychol., No. 8), pp. 30–44.

[3] Banister, H. A. The effect of binaural phase difference on the localization of tones at various frequencies. *Brit. J. Psychol.*, 15, 1925, 280–307.

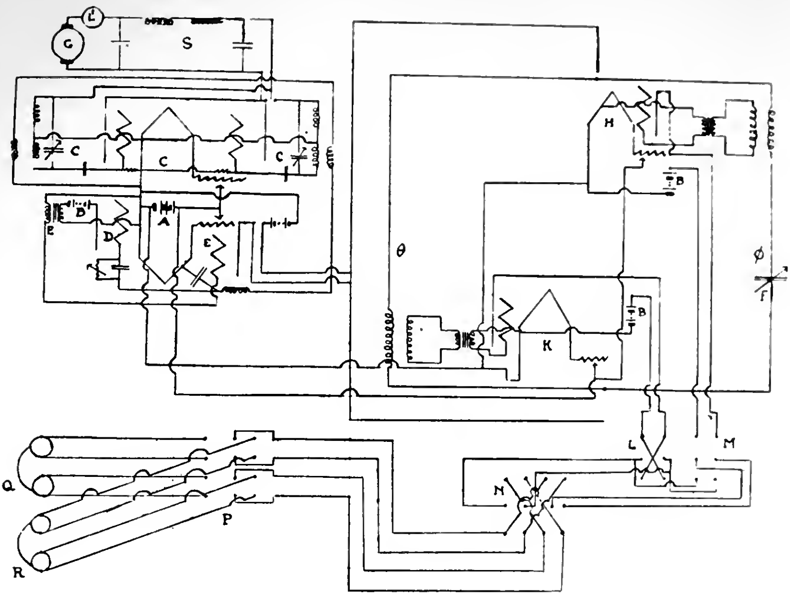

Fig. IV. Diagram of entire apparatus *

A. "A" battery
B. "B" battery
C. Heterodyne oscillators
D. Detector tube
E. Amplifier
F. Block of fixed condenser shunts with variable vernier
G. Generator (110 volts)
H. and K. Differential amplifiers
L. Polarity switch
M. Zero switch
N. Reversing switches
R. Experimenter's phones
Q. Observer's phones
S. Filter circuit
L¹. 40-watt lamp

* Note: For the sake of simplicity the plates are represented by straight lines, the first four tubes are represented as being supplied with only two filaments, and wires to the filament circuit are represented as connected directly with the filament. The tubes have therefore not been circled.

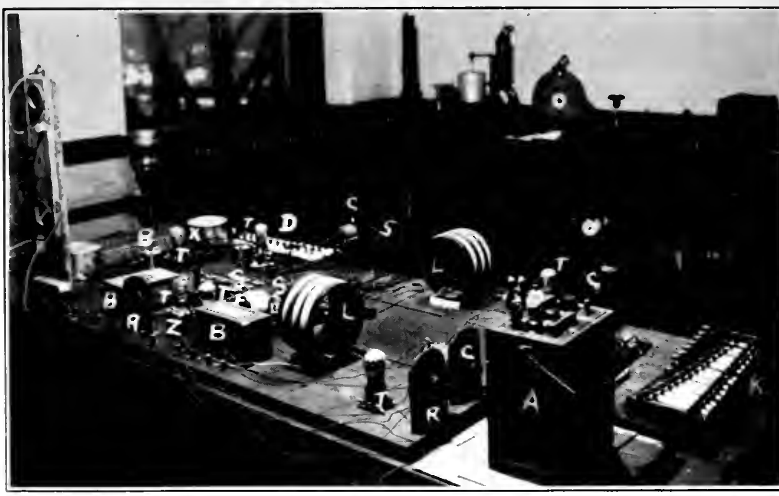

Fig. V. View of apparatus with the oscillator shields removed

A. "A" battery
B. " B " battery
C. Variable condenser
D. Right, left, zero, and polarity switches
F. Filter
K. Koenig bars
L. Oscillator coils
R. Rheostat
S. Series of fixed condensers with switches
T. 201-A vacuum tube
V. Variometer
X. Shielded air core transformer
Z. Transformer

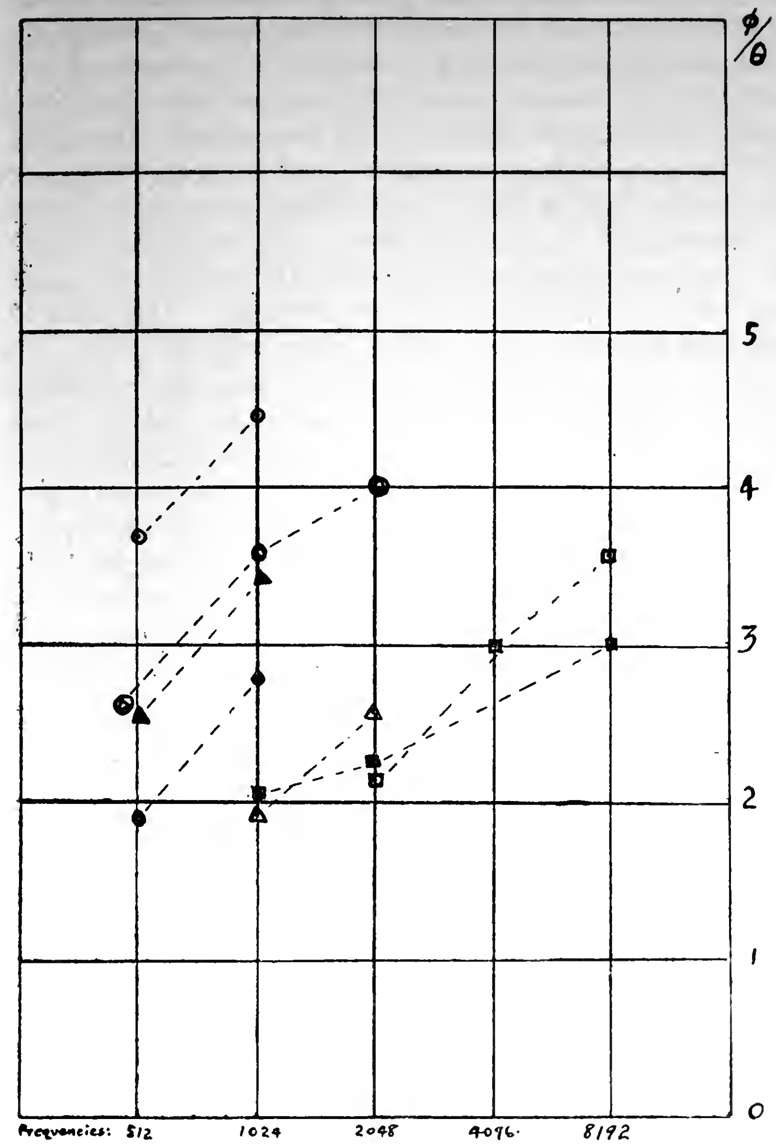

Fig. VI
CHART SHOWING INCREASING VALUE OF ϕ/θ WITH INCREASING FREQUENCY

he from any cue know how great a displacement to expect. After listening a few moments to the receivers as operating, the *O* recorded his judgment to the nearest 45°. The curves plotted in Fig. VI represent the results for seven different *O*s. Each symbol represents the average of 30 observations. The actual electrical phase angle is denoted by ϕ and the observed angular displacement is denoted by θ.[4] The tendency shown is an increase of the ratio with increase of frequency. If the observed deflection were proportional to the time intervals involved, one would expect the ratio to increase with the frequency. This tends to show that the phase effect is really a submerged time effect.

Discussion of results. Since this study was mainly to determine the limiting frequencies of phase effect, and the chart presented has dealt almost entirely with the ϕ/θ relationship and was not intended to exhibit the frequency limits found, it was thought advisable to make a special series of experiments, qualitative in nature, in attempt to establish the extreme limits of pitch wherein phase is operative. For the confirmation of the upper limit a group of naïve *O*s who had had little if any experience in experimental psychology, and had never seen the apparatus. The telephone receivers were mounted on standards and padded to prevent rattling and vibration. The chair was placed midway between the two standards. In each case the distance between the *O*'s ears and the telephone receivers was carefully equalized. This distance was usually 24 cm. The *O*s were blindholded while in the room and did not know what was used as a source of sound, or where the source or sources were located. The only instructions to the *O* was to say " left " when the sound came from the left and " right " when the sound came from the right. In each case 135° settings were given. As the test frequency 8,192 cycles was selected, since on the basis of preliminary experiments which had already been made with this apparatus, this frequency had been judged to be the approximate upper limit of phase effect. Under these conditions 18 *O*s gave

[4] These symbols should not be confused with those already used in quite another connection on p. 162.

a series of ten observations each. In only two cases out of 180 was there a failure to make the response which corresponded with the actual phase setting. In only one of these cases the *O* responded "right" when the left setting had been given. In the only other case the *O* responded "same" when the left setting had been given and the right setting had just been given the time before. Each *O* was asked to estimate the distance from the source or sources. The average estimate was 15 in. Two *O*s would not attempt to judge the distance, two responded "close," and one "right here." When questioned as to whether more than one sound was heard at a time, four *O*s stated that they heard only one sound for any one setting. The only *O* who stated that two sounds were always heard was slightly deaf in her left ear due to an abscess. The other responses ranged from the two extremes cited. If a general statement were to be made, it would be that in the great majority of the cases (about four-fifths) the two tones were perceived as one.

Another series of trials was then made. In this case a second group containing 16 naïve *O*s were brought into the room blindfolded as before, one at a time. This time the "head-set" method was used and the tone was still maintained at 8,192 cycles. Out of the 160 observations three errors were made. In one of these three cases the *O* responded "right" when the setting was left. In the other two cases the *O* answered "both." Six *O*s reported hearing only one tone and in the remaining cases the judgments were confused.

The pitch was next set at 256 cycles. A third group, consisting of 16 naïve [5] *O*s, all blindfolded, were subjected to the experiment. The method used was the one previously described in the case of the first group, namely, the open air method. Of 160 observations made in this instance, four mistakes were recorded. Left and right were confused twice, and the response "both" was given twice. Seven *O*s stated that they only heard one tone,

[5] It should be stressed that the word naïve always has the same meaning here—the person had not been in the room before and had not participated in any other part of the experiment.

five heard two tones, and four gave very indefinite answers. The distance of the sounds reported varied from 3 to 19 in.

Results obtained at 10 cycles using five trained *O*s show that right and left are not confused at this frequency since no mistakes were made. One ear-phone seemed to remain inactive in all cases, although separate examination always revealed the illusory character of the phenomenon. The apparatus devised gave a very definite and deep tone, or hum, at 10 cycles which no one without a definite gap in his hearing range could fail to hear. The phase effect was also confirmed at 20 cycles, 60 cycles, and 128 cycles. Further, when the pitch was gradually changed from 10 cycles to 8,192 cycles, and the capacity, but not the phase angle was kept constant, no lapses of the phase effect occurred, *i.e.,* the two continua were coextensive. The writer feels that the ability to get and maintain a 10 cycle tone whenever wanted and with enough volume to make it unmistakable, and with no overtones to blur the effect, reveals the possibilities of the apparatus.

Theoretical significance of results. The subject may be approached from two angles: (1) Theoretical justification for the occurrence of the wide frequency range of phase effect; and (2) the bearing of the results on the theory of audition. When one comes to consider the possibilities of a phase effect being detected at 8,192 cycles from an *a priori* point of view there are many physical, physiological, and psychological factors which should not be ignored. First, let us consider the physical factor of wave length. The length of the sound wave at 8,192 cycles is 4 cm. or 1.6 inches. If the sound to the right ear led the sound to the left by a phase angle of say 175 degrees, one wave would be approximately 2 cm. or three-quarters of an inch in advance of the other. The corresponding time difference would be approximately .06 σ. A great deal of theorizing has been done to the effect that the reason we cannot perceive phase changes above a certain frequency—a statement which varies with the author—is that sound shadows take care of the localization of the high tones. Hypotheses of this kind require no answer except the experimental results. Sound shadows could not

influence the results in the present experiments, at any rate, since with the head-set method there is no possibility of their appearance, and in the open air experiments each of the tones influenced its respective ear, and the volume was kept down to a point where it was impossible for echoes to enter into the situation. Another physical factor that should be considered is the telephone receiver. The receivers used were of advanced type as regards sensitivity, matching of tone and impedance. It is difficult to state, however, what part segmental vibration of the diaphragms and minute differences of other kinds may have had on the detection of phase effect at higher frequencies than the limit stated. Four receivers of the type used were at hand and these were frequently interchanged with no apparent effect on the results.

Klemm [6] has discussed the reflex connections between the muscles of the inner ears. There is much evidence of synergy between these two sets of muscles. According to Klemm (*op. cit.*, p. 75), "Urbantschitsch often noticed after operations that an improvement of hearing appeared on the normal side. This improvement of hearing cannot be considered as the removal of reflexive contraction since the inner muscles of the diseased ear have escaped injury." The conclusion reached by Urbantschitsch was that there is a sensory connection by virtue of which the excitation of the sensitive trigeminus fibers on the one side alone increases the irritability of the other. Several German writers take the view that the reciprocal effects noticed between the two ears are of peripheral origin. Bone conduction and conduction through the eustachian tube seem to offer little hope of explanation. Perhaps the best lead may be had from reading Sherrington,[7] on the facts of reciprocal inhibition and refractory phase. If the reader will notice the delicate temporal discriminations involved in this work, close analogies with auditory

[6] Klemm, O. Über den beidohrigen Hörens. *Arch. f. d. ges. Psychol.*, 38, 1919, 71–114.

[7] Sherrington, C. S. *The Integrative Action of the Nervous System.* New Haven, 1906. (*V.* p. 83ff.)

phenomena will be seen. There is no need of postulating special phase or time receptors. There are a sufficient number of receptors and nerves already known to account for the effect if only the manner of operation were known.

It is in the field of psychology itself, however, that the results obtained with this apparatus receive the most adequate confirmation. Klemm[8] (pp. 127–128) shows that time differences as low as 0.002 σ were distinguished by an *O*. This value was obtained by the use of the Helmholtz pendulum, revolving drum, telephones, and apparatus for producing the sound which was used. He verified this by making .5 cm. shifts in the distance from the source to the ear on one side of the *O*, and the same result was obtained (*op. cit.*, p. 135). He does not state that the time was discriminated directly but that the simple objective field of hearing shifted sufficiently under the influence of the experimental changes to indicate the effect of the time difference. Von Hornbostel and Wertheimer[9] found the threshold of change for the subjective field to be in the neighborhood of 0.003 σ, a figure very close to the value set by Klemm. If phase localization is conditioned by the implicit time interval it seems that there is here sufficient evidence that setting the limit at 8,192 cycles is not at all unreasonable from a theoretical standpoint.

The lower limit has not been touched on in the foregoing discussion. Supporters of the phase theory have always insisted that it was effective for the graver tones. Bringing the limit down to 10 cycles does not therefore need any theoretical justification.

Conclusions. The group of experiments performed in this study show:

1. That there is a phase effect within the range studied (10–

[8] Klemm, O. Über den Einfluss des binauralen Zeitunterschieds auf die Lokalisation. *Arch. f. d. ges. Psychol.*, 40, 1920, 117–146.

[9] Hornbostel, E. M. v., and Wertheimer, M. Über der Wahrnehmung der Schallrichtung. *Sitzungsberichte der Preuss. Akad. der Wissensch.*, 1920, 388–396.

8,192 cycles). The ability to recognize this effect is not limited to trained *O*s but is equally perceptible to naïve *O*s.

2. There is a ϕ/θ relationship within the range studied.

3. ϕ/θ is proportional to the implicit time difference in the sound waves under the conditions of phase lead and lag. While ϕ/θ is approximately linear for any one *O* it varies with the individual *O*.

4. The phase effect is continuous within the range studied.

SOME TEMPORAL ASPECTS OF SOUND LOCALIZATION [1]

by

Otis C. Trimble

Theoretical considerations; survey of previous investigations; problem; the technic; apparatus; an experimental survey of known temporal aspects: the localization-threshold, the course and characteristics of the fused sound, the normal binaural time-threshold; other temporal aspects: the dichotic sounds, other auditory illusions; localization with changes in absolute intensity; the influence of sound shadows on localization; experiments to check the technique: localization toward the right and toward the left, localization with the sound sources interchanged, localization with the telephone receiver technique; summary and conclusions; theoretical interpretations; bibliography.

Theoretical Considerations

Localization of sound in auditory space has been studied from many different angles since *Weber* (28) opened the field for investigation in 1848. Phenomena of both monaural and binaural localization have been studied; distance, volume, and direction having been variously considered. But since the perception of direction, the primary dimension of auditory space, is mainly a function of the two ears, binaural hearing has been the more thoroughly investigated. Most of this work has been done with continuous sounds (24) (17) in which wave-phase, intensity, and time of arrival of corresponding parts of waves at the ears have been considered, separately or collectively. Only in recent years has binaural localization been studied by the use of discrete, diotic sounds. From this point of attack the field of auditory localization is relatively unbroken.

Sound localization has become a specialized branch of acoustics (19) (20) (5) in which the physiologist, the physicist, and the

[1] I wish to express acknowledgments to Professor Christian A. Ruckmick under whose direction this investigation was made: to Dean Carl E. Seashore for many valuable suggestions; and to the observers who so generously gave of their time.

psychologist are alike interested in so far as theory to account for localization is concerned. Of numerous theories that have been advanced, the intensity-theory, the phase-theory, and the time-theory are most generally considered singly or in combination, as adequate to explain binaural localization.

The intensity-theory, one of the first to be introduced, holds that localization is a function of the ratio of the intensities at the two ears; *i.e.,* if two equally intense sound stimuli are presented at the ears, simultaneously, the localization of the phantom source is median; and, as the intensity on the side of either ear is gradually increased or decreased, the localization of the phantom gradually changes from the median plane to the aural axis on the side of the more intense sound; reverse conditions would reverse the course of the phantom. Briefly stated, localization is on the side of the ear which receives the more intense sound.

The phase-theory, now a quarter century old, holds that the localization of the phantom source is on the side of the ear receiving the leading phase when two tones of the same frequency and equal intensities are brought to the ears. If like phases arrive at the ears simultaneously, localization is in the median plane. When the phase leads to either side, the phantom source shifts to that side and describes an arc from the median plane to the aural axis as the leading phase increases in lead to exactly one-half wave length, at which time the two tones are again in phase. Further increase in lead after this point changes the localization to the other side, and the course is described in reverse order, the phantom coming again to the median plane when the leading tone is just exactly one wave-length ahead of the other.

The time-theory, the most recent theory to account for "rightness" and "leftness" in localization of sound, asserts that localization is toward the side of the ear receiving the prior of two sounds, when two discrete sounds that are qualitatively alike and equally intense are presented, one at each ear, with a very slight temporal disjunction. If two sharp sounds of like quality and equal intensity are exposed simultaneously to the two ears, a

single fused sound is localized in the median plane. If the sounds are exposed so that one is prior to the other by a gradually increasing interval of time, the fused sound gradually moves from the median plane to the aural axis. The reverse temporal series would reverse the course of the fused sound.

Since the three theories assert essentially the same claim, *viz.*, that localization is due to its peculiar factor, the question naturally arises as to the relationship existing between time, phase, and intensity as effective agents in localization. What is the element, common to phase, time, and intensity, that is responsible for binaural localization of sound? A comparative study of established phase-, intensity-, and time-aspects of sound localization should throw some light on this question. Numerous phase- and intensity-aspects are already well established, while but a few temporal aspects are, only partially, known. In the following pages some temporal aspects of sound localization are set forth. On the basis of these, along with already known time- and the established phase- and intensity-aspects, attempt is made to define the element common to phase, time, and intensity that is responsible for binaural sound localization.

Survey of Previous Investigations

Klemm (13 and 14), in 1919, developed a technique for the study of binaural sound localization which made use of discrete, diotic sounds. The temporal difference of arrival of the sounds at the ears was the factor studied in relation to localization. He used telephone receivers to generate the sound impulses. To obtain sounds that were qualitatively alike the receiver membranes were stretched until the stimulus sounds were perceived to be alike in quality. The intensity factor was controlled by introducing resistance into the circuits through the telephone receivers. The apparatus with which he controlled the time factor consisted of a Wundt pendulum, which was later replaced by a Helmholtz pendulum, the fall of which broke or closed the circuits through the receivers. He employed both the direct, *i.e.*, the receivers placed in direct contact with the ears, and the open

air methods of delivery of the sounds. This was the first single impulse technique for the study of binaural localization of sound, neglecting the contributions of *Weber* (28), *Fechner* (7), and *von Kries* (15), who studied binaural hearing by the use of watches that ticked slightly out of time.

Klemm found that when two discrete, diotic sounds were presented simultaneously, one at each ear, that a single fused sound, "subjective-hearing-field," was localized in the median plane. This fused sound remained in the median plane until simultaneity had been departed from by a time-difference of $2\,\sigma\sigma$, which Klemm designated as the threshold of binaural localization of sound. Above this threshold the sound moved slowly from the median plane to a maximal lateral position on the side of the prior sound, where it remained until the threshold of binaural time-perception $2\,\sigma$ was reached. The sound below the localization threshold was described as appearing stronger and higher in pitch during time-simultaneity than during time-difference. As simultaneity was departed from, the sound took on a broadening aspect which continued above the localization threshold, finally appearing as illusory movements which seemed to bridge the ears, or as two or three sounds just at the threshold of binaural time-perception at which the binaural sound divided into two dichotic sounds. These dichotic sounds, one stronger, more definite and voluminous than the other, moved away from the ears as the temporal disjunction was further increased.

Klemm apposed time and intensity, and distance and time, and found that a time-difference could be equalized by either a distance-difference or an intensity-difference.

In accounting for the effectiveness of time-difference in binaural sound localization, Klemm suggested that time might be reduced to intensity, or to phase and then to intensity, on the principle of interference due to bone-conduction. Or, what seemed more probable to him, that the sound impressions, however created, resulted in "movement" along the auditory nerve-pathway from ear to ear; and that a localization was but an almost "congealed motion."

Von Hornbostel and *Wertheimer* (11), working together and independently of Klemm, came out with a different single impulse technique for sound localization, at about the same time that Klemm reported his technique. They employed a short, sharp knock-noise which was received by two funnels, or microphones, and conveyed to the ears through tubes, or over electric circuits, to telephone receivers at the ears. The time factor was controlled by varying the distance of the sound source relative to the funnels, or microphones; through a movement of one or both of the funnels, or microphones, relative to the sound source; or by inserting telescope-cylinders between the telephone receivers and the ears. They depended upon resonance in the tubes to equalize the differences in intensity, due to the distance-difference of the sound source from the ears.

These investigators found that by presenting single diotic sounds simultaneously, or with a very slight temporal disjunction, up to 30 $\sigma\sigma$, a single fused sound was localized in the median plane. As the temporal disjunction was gradually increased the fused sound moved gradually to the aural axis on the side of the prior sound when the temporal disjunction was 630 $\sigma\sigma$. They reported that the relationship between angular displacement and time-difference during the period of the shift of the fused sound from the median plane to the aural axis was linear. Above the localization threshold the fused sound remained whole, and was localized on the side of the prior sound at the 90° position for a time, after which it fell back about 30° toward the median plane. Localization in the unnaturally large temporal disjunctions was indefinite and uncertain.

They discovered that the angular distances of the stimulus sounds from the median plane need not be equal to obtain a fused sound localized in the median plane when the stimulus sounds were presented simultaneously. They apposed time and intensity and concluded that only when the prior sound had been completely blotted out, by making it less and less intense, did the localized sound jump suddenly to the other ear.

They defined phase-difference as a special case of time-differ-

ence, denied the adequacy of intensity as an agent in localization, and held that localization could be completely accounted for in terms of the time-difference of arrival of sounds at the ears. The physiological processes, the basis of direction perception, they suggested, were central as opposed to peripheral. These investigators were first to define the time-theory of sound localization.

Hecht (10), in 1922, reviewed experiments (whether they were his experiments or those of other investigators could not be determined from his report) in which localization had been studied by the use of short noises as stimulus sounds. He described no technique. Hecht held that the fused sound began to be localized outside of the median plane, on the side of the prior sound, when the temporal difference of arrival of the single wave fronts at the ears was 30 $\sigma\sigma$; that the angular displacement was proportional to the time-difference of arrival of the sounds at the ears until the disjunction was .6 σ, when the fused sound came to the aural axis; that the fused sound remained localized at the 90° position until the binaural threshold, 1.2 σ, of time perception was reached.

He concluded from his experiments on direction perception in "stationary sound fields" that in such fields created by continuous sounds of very high frequency intensity was the determining factor in localization; that time was the effective agent in fields of very low frequency; and that time, phase, and intensity were all effective in fields of frequencies ranging between the very high and the very low. He described localization as a "capacity of the brain."

Wittmann (30), in 1925, introduced another single impulse technique. He set up the stimulus sounds by means of an eccentric device which controlled the up and down movements of platinum points which, dipped into separate mercury cups, thus closing or breaking electric circuits through telephone receivers. The mercury cups could be adjusted up and down so that the circuits were closed or opened simultaneously or with slight temporal disjunction, thus making it possible to regulate the priority

of the sounds. The intensity factor was controlled by means of variable resistances in circuit with the receivers. He used the open air method of delivery of the sounds to the ears.

Wittmann found that a single fused sound was reported in the median plane when the stimulus sounds were presented simultaneously or with a time difference ranging up to 30 $\sigma\sigma$. A slight deviation from the median plane to the side of the prior sound was reported for temporal differences ranging from 30 to 40 $\sigma\sigma$. The maximal side deviation occurred when the time interval was 2.6 σ. An apparent movement of the sound from one ear to the other was reported when the temporal disjunction was about 16.5 σ. The fused sound divided into two sounds when the interval ranged above 16.5 σ. As the fused sound moved from the median plane toward the aural axis, it changed in both loudness and quality, being low, or deep, and loud in the median plane, and higher in pitch and fainter at the extreme lateral position. Wittmann suggested that the relationship between angular displacement and time-difference of arrival was approximately rectilinear, as the fused sound changed from the median plane position to the aural axis.

This investigator employed time and intensity interchangeably and discovered that both could supplement each other in their effect upon localization, or that they could to a varying degree neutralize each other. He concluded that, in answer to von Hornbostel and Wertheimer, sound localization depended upon intensity- as well as upon time-difference of arrival of the stimulus sounds.

Wittmann described localization phenomena as "objects-heard" in auditory space in comparison with "objects-seen" in visual space.

Banister (2), in 1926, employed a single impulse technique in which the sound impulses were set up by breaking electric circuits from a four-volt storage battery, through telephone receivers without diaphragms. When the circuits were broken, very faint clicks of no determinate pitch were heard. By means of a Keith

Lucas contact breaker the time-interval between the clicks was adjusted.

Banister found that when the clicks were simultaneous, one sound was localized in the median plane. If short intervals occurred between the two clicks, one sound was still heard, but it was localized to the side which was first stimulated. As the temporal interval was further increased, the sound appeared to move more and more toward the side first stimulated, till, at a temporal interval of about 1.75 σ, the fused sound split into two sounds, one on either side, the second being much fainter than the first. Yet, after this division, given a suitable attitude on the part of the observer, a single binaural sound might be localized. A further increase of time-difference gave two sounds that were equally intense. The fused sound, whatever its localization, did not vary in loudness, though it varied in position and sometimes in volume.

This investigator concluded (2, p. 147): "It is impossible at present to say how this factor of localization is finally decided, but the first impulse has a certain priority over the second, and it is this priority which determines our judgment of the direction from which the sound appears to come. The effect is not analogous to the inhibition of one impulse by a previous one which has passed along the same nerve fibre. In this case, if the second impulse follows the first within the Refractory Period it is denied passage. This does not occur when the impulses are along corresponding fibres from the two cochlea. Such impulses are integrated, and the result of the integration gives the sound, among other things, this new attribute of varying direction."

Bennett (4) has recently, in 1927, published his technique for measuring the efficiency of the ears as a means of detecting short time-intervals. He used telephone receivers to generate and deliver the sounds to the ears. His time-apparatus consisted of a revolving disc on which was a metal band which delivered to contact brushes charges which caused the receiver membranes to vibrate. The relative distance between the contact brushes

could be adjusted so that the temporal interval between the impulses could be regulated from 0 to .0025 second.

He concluded that localization for the average observer was a matter of guessing when the temporal interval ranged below approximately .1 σ; and that " rightness " and " leftness " could be distinguished for certain at 1 σ difference.

Kester (12), in 1926, in studying phenomena of movement in localization, employed a technique in which telephone receivers were used to generate the stimulus sounds. The receivers were mounted on movable pulleys which hung from parallel cords, approximately 6 m. apart, which were in the horizontal plane of the ears, and parallel to the median plane. He did not reduce the stimulus sounds to qualitative likeness. The intensive factor was controlled by varying the strength of the respective currents through the telephone receivers, and by adjusting the distance of the receiver membranes from their respective magnets. The temporal interval was varied by means of a Schumann's *Zeitsinnapparat.*

Kester's investigation was concerned with the localization of both sound sources. He studied the apparent localization in relation to the actual position of the sound sources as the temporal interval between the two sounds was varied, and as the relative positions of the sound sources changed.

His technique can hardly be called a single impulse technique for binaural localization of sound since he did not control the qualitative factor.

Other investigators, notably *Starch* (23) who thoroughly investigated localization in the horizontal and the vertical planes of the ears, and *Allers* and *Schmiedek* (1) who studied the influence of visual after images and kinaesthetic sensations upon localization, have employed a single sound source which was shifted about, at a constant distance, in the planes of the ears. But these can not be considered as essentially single impulse techniques for the study of the influence of time-intervals on localization, since the intensity factor, with its attendant qualitative and extensive aspects, was not controlled.

The Problem

The foregoing survey shows general agreement concerning the fundamental aspects of binaural localization of sound as conditioned by the temporal factor; but the various investigators have assigned widely discrepant values to the critical points in the temporal series. Also, the investigations appear more or less incomplete or fragmentary; and the results have been variously interpreted by the different investigators.

In this investigation, phenomena of binaural localization of sound in relation to the temporal factor have been studied with a three-fold purpose: (1) to harmonize, or account for, the discrepant values assigned by previous investigators to the critical points in the temporal series; (2) to set forth more completely the temporal aspects of binaural sound localization; and (3) to define more exactly the effectiveness of the factor of time-difference in binaural localization of sound.

With a single impulse technique which avoided conditions of resonance and the attendant possibilities of phase-effect, such as are present in telephone receivers and conducting tubes, the following experiments were performed:

Series I. To determine the localization threshold, *i.e.*, the temporal interval above which the binaural sound is localized toward the side of the prior sound.

Series II. To study the course and characteristics of the binaural sound perceived below the binaural threshold of time-perception.

Series III. To define the binaural time-threshold, *i.e.*, the temporal interval at which the fused sound divides into dichotic sounds.

Series IV. To study the courses and characteristics of the dichotic sounds.

Series V. To study the course and characteristics of the binaural sound that appears above the binaural time-threshold.

Series VI. To determine the function of different absolute intensities in localization due to the temporal difference of arrival of the stimulus sounds.

Series VII. To find the influence of a shadow larger than that cast by the head, upon localization due to time-difference.

Series VIII. To check the technique
(1) By comparing right and left localization,
(2) By studying localization with the sound sources interchanged,
(3) By studying localization with the telephone receiver technique.

The Technique

The first problem in the employment of a single impulse technique was that of the sound sources, *i.e.,* by what means could clear, short sounds be set up. Consideration of various possible means led to the use of electric sparks to generate the sound impulses. The use of sparks necessitated the open air method of delivery of the sounds, or else the use of tubes, which were electrically non-conducting, to convey the sounds to the ears. The use of tubes introduced conditions of resonance in the outer auditory canals as well as in the tubes. Also, tubes, because of damping effects of friction and reflection required relatively strong sparks to generate the sounds, thus increasing the chance for irregular slippage at the spark gaps. In order to avoid these conditions, the open air method was employed in a sound proof room, which made it possible to use very weak sparks.

Since the stimulus sounds must be qualitatively alike and of equal intensity, conditions on the side of either ear must be as nearly identical with those on the other as possible. To meet these conditions, the sparks which set up the sounds at the two ears were generated by breaking currents of 4 amp. at 2 v. through induction coils of identical type. Special condensers were connected across the break points of the connections through the primaries. The spark gaps were adjusted to .5 mm. A headrest, which marked the *Os'* position was so attached in the system that the spark gaps were kept at a constant distance from the ears of the *Os*. The *Os'* position was fixed equidistant from all sides of the cubical room which was lined with acoustic absorbing material. As a further precaution against reflection, acoustic absorbers were mounted just back of the spark gaps. These arrangements gave fair assurance that the sounds were qualitatively alike and of equal physical intensity.

The *Os* were seated, singly, in the observation cage while *E* in a distant room controlled, by means of a pendulum device, the time of arrival of the sounds at the ears. As a signal to the *Os, E* disconnected the light in the sound proof room a few seconds before the stimulus sounds were released. The room was

left darkened during the exposure of the sounds, so that the *Os* could work under almost ideal conditions for hearing; only their organic sensations were present to distract, neglecting, of course, the sensations of blackness and quietness to which the *Os* soon became habituated. The light was turned on immediately after the exposure so that the *Os* could record their observations.

After a brief period of trial experimentation testing the apparatus and trying out methods of exposure, it was decided that results would be more exact and could be more easily checked if the sounds were exposed in serial rather than in random order, because the effect of movement or change would be more noticeable since there would be no lapping back or crossing over to the opposite side. Also, the last sound each time in the series could become a point of reference, while in the random order the median plane or the extreme lateral position must always be the point of reference. The series were run in ascending and descending order, *i.e.*, the temporal disjunction of the stimulus sounds was gradually and regularly increased from zero to the required magnitude, or the reverse was the case. In order to eliminate the error of expectation and to equalize differences in localization due to differences between the ears or to chance differences between the stimulus sounds, ascending and descending series were run on both sides, and the estimates averaged.

This investigation required *Os* with balanced ears, *i.e.*, ears equally susceptible to sound impressions. The *Os* had to be trained to localize in terms of degrees from the median plane and to analyze their auditory experiences. The first *Os* to be trained were *S, M,* and *H*. After the investigation had been in progress about four months, two other *Os, Ho* and *Ha,* were trained; and later still, about five months, three other *Os, L, Ml* and *Hd,* were trained for the investigation. All of these *Os* were seniors or graduate students, majors in psychology. They were not familiar with the problem and the facts of sound localization. *H* and *Ha* observed in all stages of the experiment. *S*

and *Ho* dropped out in the middle stages of the investigation. *M* observed only in the very first part of the experiment. The last three *Os* to be trained were used in the final stages of the investigation.

The only test applied in the selection of the *Os* was, whether or not a single fused sound was perceived localized in the median plane when the stimulus sounds were presented simultaneously. Of ten persons who were asked to serve as *Os* in the investigation, two were found by the test to have unbalanced ears. One always heard both stimulus sounds, while the other perceived a single fused sound localized far to the side of the median plane. They had had audiometer tests, and knew beforehand that their ears were unbalanced. This was a good test of the method of selecting the *Os*. To further check the method, audiometer tests were given to *S, Ho,* and *Ha* who were found by this means to have balanced ears.

The first *Os, S, M,* and *H,* were trained with the phantom sound in the median plane region, *i.e.,* in the median plane exactly or a few degrees to either side, in order to avoid suggestion as to the course of the phantom until they were accustomed to the experimental conditions and to some extent familiar with the phantom, or fused, sound. The very first instructions to these *Os* were: "You will hear a sharp sound. Please record R, if it is to the right of the median plane; L, if to the left; and M if in the median plane." After several periods of observation under these instructions, the *Os* were told that the series would be lengthened, but that they were to report as directed in the previous instructions. *E* called for no other reports, but he took down the observations that the *Os* spontaneously reported. Soon the *Os* had made the fundamental observations that the sound moved about through what approximated a semi-circle, a 90° arc on either side of the median plane; and that it divided into two sounds, one on either side, when it was at the maximal lateral position as ascending series were run; and that these two sounds fused when the series were run in descending order.

The other *Os* were introduced to the complete situation and

asked to report their auditory experiences. After a few observation periods, they had made the fundamental observations listed above. The training of all the *Os* in the matter of estimating the angular displacements was simply a matter of practice. Specific instructions to the *Os* during this period of training were: "Attend closely to the fused sound as it changes its position from time to time, noting its angular distance from the median plane. Record R or L, with your estimate of the angular displacement from the median plane each time. When you hear two sounds, record the direction and the angular displacement of both." In order to facilitate training, the temporal increment, or decrement, was kept constant throughout the series. The increment, or decrement, was taken large enough to bring about a perception of change in the position of the phantom source at each step in the series.

Throughout the investigation, when estimates of angular displacement were called for, the temporal increment, or decrement, was the same as that employed in the practice series; but the length, starting point, and the direction of the series were so varied that the *Os* could not follow any preconceived plan.

Certain checks and precautions were exercised consistently. The currents were checked regularly to see that voltage and amperage were held constant. The induction coils were occasionally crossed so that the one which had generated the spark at the one side now generated the spark at the other. What amounted to the same thing, the *Os* were reversed in position so that the left ear was at the right plug and the right ear at the left plug. This was done to check the stimulus sounds for qualitative and intensive differences. Checks of the zero point were always made when there had been chance for disturbance of the apparatus. After each series, the *Os* were allowed a few minutes for relaxation, during which *E* recorded the order of the series and checked the reports of the *Os* to see if any exposures had been neglected. The length of an observation period was thirty minutes, or less, more often less, in order to avoid the element of fatigue.

Apparatus

The apparatus consisted of a receiving, a sending, and a connecting unit. The receiving unit, Fig. I, was a system in which two ordinary commercial spark plugs, the points of which had been filed down to prevent irregular slippage, were mounted diametrically opposed to and facing each other. The plugs, with acoustic absorbers, were attached to rods bound perpendicularly

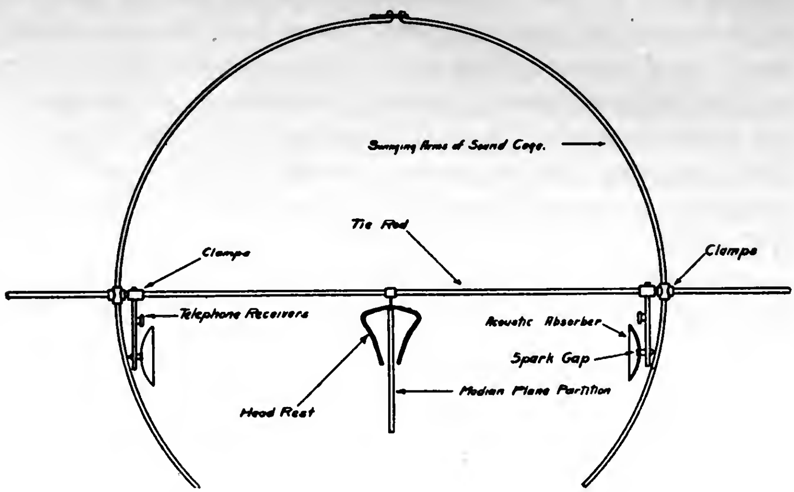

Fig. I — Receiving Unit —

to a heavy metal rod which was rigidly clamped to the swinging arms of the Seashore sound cage.[2] The perpendicular rods were adjustable, allowing the distance between the spark plugs to be varied. From the middle of the heavy tie-rod, the *O*s head rest was fixed. The signal light was placed above and slightly to the front of the head rest. During that part of the investigation which was concerned with the influence of acoustic shadows, a median plane partition one meter in diameter and covered with acoustic absorbing material, was attached to the head rest. This partition was so made and so attached in the system that the aural axis passed through the center, and perpendicular to the plane.

[2] Seashore, C. E. The sound perimeter. *Psychol. Rev.*, 10, 1903, 64–68.

Leading from each plug were two insulated wires which extended to a room adjacent to the sound-proof room in which the receiving unit was set up, where they were connected to the proper points of the induction coils. The induction coils were each connected in circuit with a two-volt storage battery, the connection through the primary coil of each being made by wires leading to the connecting posts of the contact keys of the fall apparatus of the Klopsteg chronoscope.[3] When these keys were closed circuits from the storage batteries were made through the primaries of the induction coils. Special condensers were connected across the points of these contact keys to prevent arcing at the points when the circuits were broken. This was the connecting unit of the apparatus (see Fig. II).

The time-machine, which was the sending part of the apparatus (Fig. III) consisted of the two keys of the Klopsteg chronoscope, one of which was fixed to the base of the Dunlap pendulum[4] so that it was at the lowest point of the arc described by the swinging pendulum. The other contact key was mounted on a screw device so that it could be changed back and forth in the horizontal plane of the fixed key. By means of the screw the relative distance between the contact keys could be varied by running the movable key either forward or backward in the horizontal plane. The end of the bob-arm of the swinging pendulum broke the circuits through the induction coils, and sparks were set up at the spark gaps. If the contact points were in such a position that the pendulum broke the two circuits simultaneously, the sparks were simultaneous. Any change from this relative position of the keys caused one of the sparks to be prior to the other by a temporal interval depending upon the distance between the two keys. Since the movable key could be moved either backward or forward, priority of the sounds could be controlled. It was the time of the swing of the pendulum through the very small arcs represented by the distance of the movable

[3] Klopsteg, P. E. A new chronoscope and fall apparatus. *J. Exp. Psychol.*, 2, 1917, 253–263.

[4] Dunlap, K. A new laboratory pendulum. *Psychol. Rev.*, 19, 1912, 240–245.

key from the fixed key that measured the time interval between the two sounds.

The time of the swing of the pendulum through such very small arcs was determined with the Klopsteg chronoscope. The zero point, point of simultaneity, was first determined as nearly as possible by trial and error procedure. This point was set by adjusting the mm. scale, attached to the base of the screw, so

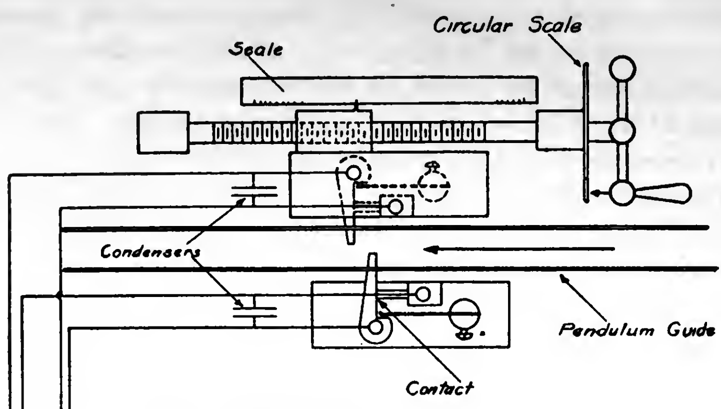

Fig. III —Sending Unit—

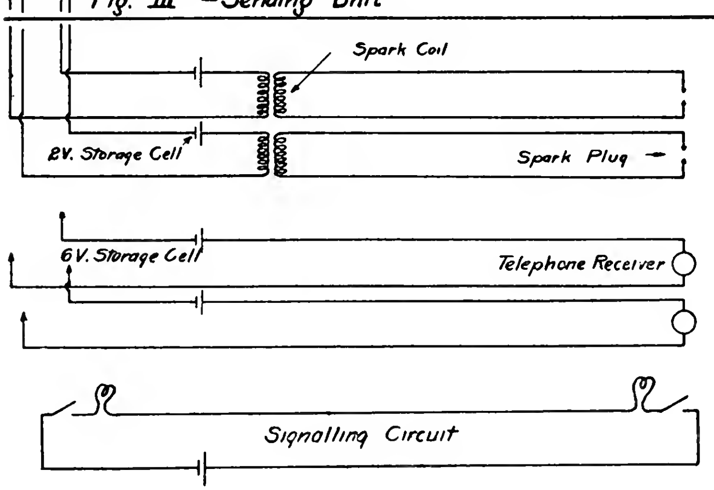

Fig. II —Connecting Unit—

that the pointer fixed to the movable key indicated the point on the scale taken as zero. From this zero point, the movable key was run backward ten turns. The time of the fall of the pendulum through this distance was measured with the chronoscope. Then the movable key was changed to ten turns forward from the determined zero point and the time of the swing through this distance was determined as before. By the formula (1) $\frac{L+R}{2T}$, in which L is the time of the swing when the movable key is ahead of the fixed key, R is the time when the movable key is back of the fixed key, and T the number of turns held constant for both R and L, the time of the swing of the pendulum through one turn of the screw was found to be 1.353 sigma. By either of the following formulas: (2) $\frac{L - \frac{R+L}{2}}{Ta}$; and (3) $\frac{R - \frac{R+L}{2}}{Ta}$, in which L, R, and T are the same as in (1) and a is the time of the swing through one turn, the correction to the obtained zero was found, and the screw device adjusted to the true zero. Either formula (2) or (3) was used when it was necessary to check the zero point.

It was found easier to regulate the time interval by the use of fractional turns of the screw than by the use of the mm. scale and pointer device. This was made possible by fixing a circular disc to the base of the screw device in such a way that the screw bolt passed through the center. The circumference of this disc was divided into sixty-four major parts which were subdivided until it was possible to read the disc in terms of 512ths. A pointer was fixed to the screw handle to indicate the position of the movable key in relation to the fixed. This made it possible to measure time-difference accurately from zero to tenths of a second.

In that part of the investigation which made use of the tele-

phone receiver technique, the spark plugs were replaced by telephone receivers, through which the circuits were connected direct, leaving out the induction coils.

An Experimental Survey of Known Temporal Aspects

From a consideration of the investigations reviewed in the introductory section, the following temporal aspects of binaural localization may be considered as known:

(1) That there is a binaural time-threshold of localization;

(2) That, above this threshold, the binaural sound is localized toward the side of the ear receiving the prior sound;

(3) That the fused sound comes to the aural axis at a certain point in the temporal series;

(4) That the binaural, or fused, sound finally divides into dichotic sounds;

(5) That the binaural sound has certain, peculiar, qualitative and intensive characteristics; and

(6) That a certain functional relationship obtains between angular displacement and time-difference.

But, it is very evident, also, that no very definite conclusions concerning these temporal aspects can be drawn because: (1) the values assigned by previous investigators to the critical points in the temporal series are at variance; (2) the investigators are not agreed concerning the characteristics peculiar to the binaural sound; and (3) the functional relationship between time-difference and localization has not been thoroughly worked out.

In order to clear up the existing discrepancies, and to define more exactly the temporal aspects listed above, a survey consisting of a series of experiments dealing with the binaural sound over different, limited ranges of temporal differences, was made.

The Localization-Threshold

The first of these experiments dealt with the fused sound in the mid-region. The purpose was to define the temporal threshold of localization, *i.e.*, to name the temporal interval above which the fused sound is localized toward the side of the ear receiving the prior sound.

For this experiment, because it was more convenient from the nature of the set-up, the sound sources were adjusted to 72 cm. from the ears. Since the experiment was concerned with the direction and not with the amount of displacement, the *O*s were instructed to report the direction, only, of the localization. In this situation the temporal series represented very short time-differences ranging from 0 to .32 σ.

In Table I is presented a summary distribution of 877 judgments, by four trained *O*s, of the localization of the binaural sound in the region of the median plane.

TABLE I. *Distribution of 877 judgments of localization in the mid-region (by percentages)*

Time difference	Median	Localization Right–left (expected)*	Localization Right–left (unexpected)*
.0000	54	—	46
.005	22	43	35
.01	25	35	40
.02	17	44	39
.04	21	50	29
.06	19	59	22
.08	27	52	21
.10	17	72	11
.12	23	70	7
.14	11	82	7
.16	8	87	5
.32	5	92	3

* By "expected" is meant that fused sound was localized as right or left, when the stimulus sound on the right or left was prior. "Unexpected" means that localization was on the side opposite to the prior sound, or to the right or left on simultaneous presentation of the two sounds.

Table I shows that localization is a matter of chance when the stimulus sounds are presented simultaneously or with a temporal interval ranging up to .06 σ. Above this point the chances in favor of "expected" localization rapidly increase until the .16 σ point is reached, at which point localization on the basis of temporal priority is fairly certain. Above this point in the temporal series, the chances for success in localization increase very slowly, being but slightly greater at .32 σ. These facts are more clearly shown in Fig. IV.

Fig. IV indicates the chances of success and the direction of the errors in localization in the median plane region. The curves

represent the data presented in the preceding table. The data were smoothed twice by the method of the moving average.

Fig. IV shows that the errors in localization on the basis of priority are approximately equally divided between median plane localization and R-L "unexpected" localization, with a slight

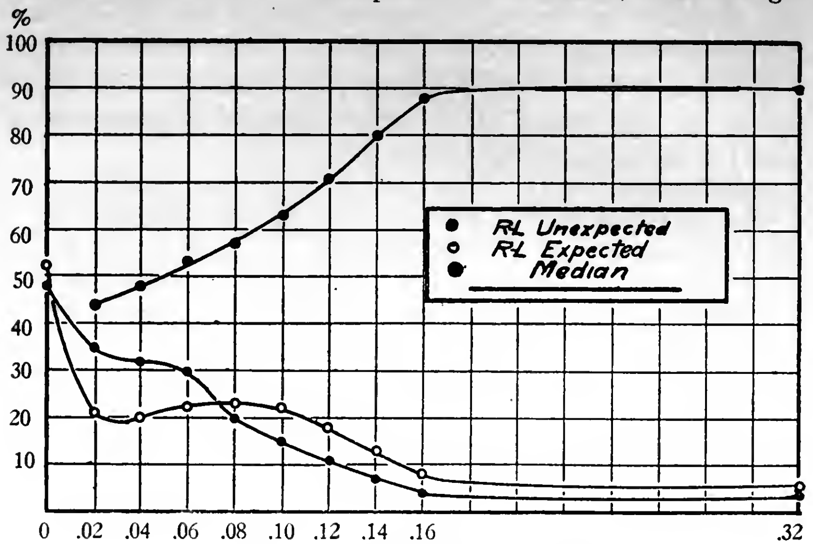

Fig. IV. Localization in the mid-region

tendency in favor of the latter when the temporal interval is very small, and in favor of the former when the temporal interval is larger than approximately .07 σ.

It can therefore be seen that the time-difference is ineffective as an agent in binaural sound localization when the temporal interval ranges below .06 σ. Below this point localization is a matter of chance, but since the displacement to the right or left is so slight, the localization may be called median.

Above .06 σ the chances for success in localization rapidly increase as the temporal interval is extended up to .16 σ which may be taken as the temporal threshold of binaural sound localization.

Errors in localization are approximately equally divided between median and R-L "unexpected" localization.

The Course and Characteristics of the Fused Sound

The second experiment of the series was concerned with the binaural sound perceived below the dichotic threshold. The aim was threefold: (1) to plot the function that localization is of time-difference; (2) to determine the course of the fused sound; and (3) to study the qualitative and intensive characteristics peculiar to the binaural sound.

As in the first experiment of the series, the sound sources were fixed at a distance of 72 cm., respectively, from the ears. The *O*s were instructed to record the angular displacement as well as the direction of the localization. They were occasionally asked to describe the various auditory phenomena. The temporal series ranged from 0 to 2.76 σ, in steps of .12 σ.

TABLE II. *Summary showing the angular displacement of the binaural sound*

Time-difference	Observers						
σ	*L*	*Ml*	*H*	*Ha*	*Hd*	*Ho*	*S*
000	X*	X*	X*	X*	X*	X*	X*
.12	8*	5**	3**	13**	15**	5**	5**
.24	10**	14**	10**	27**	24**	8**	9**
.36	22**	18**	21**	47**	31**	13**	17**
.48	28±10	32±16	29±17	43±18	39±11	20±10	23±11
.60	32±10	45±19	43±20	50±26	45±12	27±12	33±14
.72	37±11	58±16	47±23	60±21	53±17	35±12	40±14
.84	42±11	76± 9	53±24	65±18	55±14	45±13	52±17
.96	44±11	83± 7	65±23	72±17	54±15	52±13	54±19
1.08	47±10	87± 4	68±22	77±12	58±14	60±14	75±13
1.20	49±11	89± 1	73±22	78± 7	62±14	65±13	86± 6
1.32	53±10	89± 1	80±12	80± 9	64±14	73±15	86± 7
1.44	55± 9	89± 2	84± 9	83± 9	68±11	82±10	87± 5
1.56	59±10	88± 3	85± 8	88± 3	71±13	84± 7	89± 2
1.68	62± 9	89± 1	88± 3	87± 4	72±10	87± 4	90± 0
1.80	65± 9	89± 2	89± 1	88± 3	72±12	89± 2	
1.92	68± 9	89± 1	89± 1	88± 3	72±12	89± 1	
2.04	69± 7	90± 0	89± 1	89± 1	72±10	90± 0	
2.16	72± 7	90± 0	90± 0	89± 1	74± 9		
2.28	75± 5	90± 0	90± 0	90± 0	73± 9		
2.40	75± 4	90± 0	90± 0	89± 1	74± 9		
2.52	78± 5	90± 0	90± 0	89± 1	74± 8		
2.64	80± 5	90± 0	90± 0	89± 1	74± 8		
2.76	81± 5	90± 0	90± 0	90± 0	76± 8		

* X represents an unknown because of errors in localization.

** Each of these numbers represents an estimated, not a true average, because of errors, hence no mean deviation could be found.

Table II presents a summary of the estimates of angular displacement at different points in the temporal series. The estimates, with average deviations, represent averages of approximately twenty trials at each point. It is clear from this table that localization at the various points is more or less inconsistent; but that the estimates gradually increase in magnitude as the

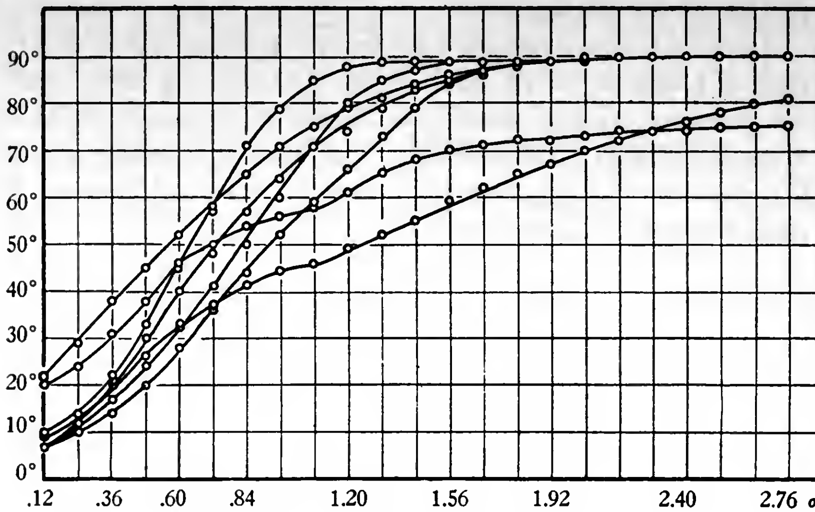

Fig. V. Localization curves for 7 *O*s

temporal interval is extended to the point at which the fused sound comes to the aural axis. The functional relationship between time-difference and localization is more clearly shown in Fig. V. The curves of localization in Fig. V indicate that the relationship between time-difference and localization is approximately linear, after the binaural sound leaves the median plane until it comes to the aural axis.

The *O*s reported consistently that the binaural sound described a 90° arc from the median plane to the aural axis on either side. The course was described as corresponding to the arc of a circle, or ellipse, until the sound had moved out to a position approximately 30° from the median plane, when it moved suddenly in toward the ear; and, with further extension of the

temporal difference, the sound was reported as moving away from the ear, along the aural axis, until it reached a stationary position. The distance of the binaural sound, as it made its course, from the head of the observer varied from a few inches to a few feet, depending on the observer.

The fused sound was variously described by the *O*s, as "a splash," "an explosion," "a dull, somewhat extended click," "a white spot on a black background," "an extended colorless spot," "a flash of light," "a point," "a something with spatial dimensions," "and auditory extent," "a wide irregular flash of whiteness," "a click," "an electric spark," "a small spherical body," "a slight tick."

Ha reported that the binaural sound remained the same in quality, but that it became less loud as it changed from the median plane to the extreme right or left. *H* observed that as the sound moved out from the median plane to the aural axis it gradually became higher in pitch, broader, more voluminous, more irregular in shape, and of longer duration. According to *Ho,* "The 'intensity' of the fused sound seemed greatest when it was in the median plane. As it traveled to either side, the sound seemed to be altered, becoming less and less 'intense' as the distance out increased." *S* stated that the binaural sound varied in both "intensity" and quality as the sound moved from the median plane to the side, being low and loud at the median plane, and changing to higher and less loud at the extreme positions. *L* reported that the sound was more "intense" when it was localized median. *Hd* observed no changes in the character of the sound throughout its course. According to *Ml,* "The median plane sound, as it moves outward seems to become more piercing and less hollow."

It follows then that a linear relationship exists between angular displacement and time-difference.

The fused sound changes in pitch, loudness, volume, and duration.

The fused sound comes to the aural axis at 2 σ.

The Normal Dichotic Threshold of Successivity, or Time-perception

The last experiment of this series dealt with the binaural sound at the dichotic threshold. The purpose was to define the exact point of successivity, *i.e.*, to name the point in the temporal series at which the fused sound normally divides into dichotic sounds.

The sound sources were fixed at various distances from the ears so that different absolute intensities would be effective in obtaining the data. This was done so that the observations by five *O*s would more nearly approximate those of a more general population because of greater differences in auditory acuity. The *O*s were instructed to report the number and the direction of the sounds heard as the temporal series were run ascending and descending across the dichotic threshold. The temporal series ranged from 2.76 σ to 5.10 σ, in steps of .12 σ.

When an ascending series was run, the point at which two sounds were first reported was taken as the threshold of successivity; and when a descending series was run, the point at which two sounds were last reported was taken as the threshold value. These values are summarized in Table III. This table represents the values grouped around the mid-points of class intervals, in steps of .36 σ.

TABLE III. *Frequency distribution of 575 values, reported by seven Os, of the threshold of binaural time-perception*

σ, Time-difference	Values	σ, Time-difference	Values
.24	4	2.76	99
.60	17	3.12	77
.96	21	3.48	17
1.32	40	3.84	24
1.68	55	4.20	10
2.04	82	4.56	3
2.40	124	4.92	2

Table III shows a wide range of judgments which approximate a normal distribution, as is more clearly shown in Fig. VI.

The average value of the point in the temporal series at which the binaural sound divides into dichotic sounds was found from the distribution in Table III to be 2.36 σ, with a standard devia-

tion of .98 σ. Since the values were distributed over such a wide range, with such a large standard deviation, the different absolute intensities might be considered as unduly influencing the range of the distribution. This problem was met by finding the range of the values reported when the strongest and the weakest intensities were employed. In case of the strongest,

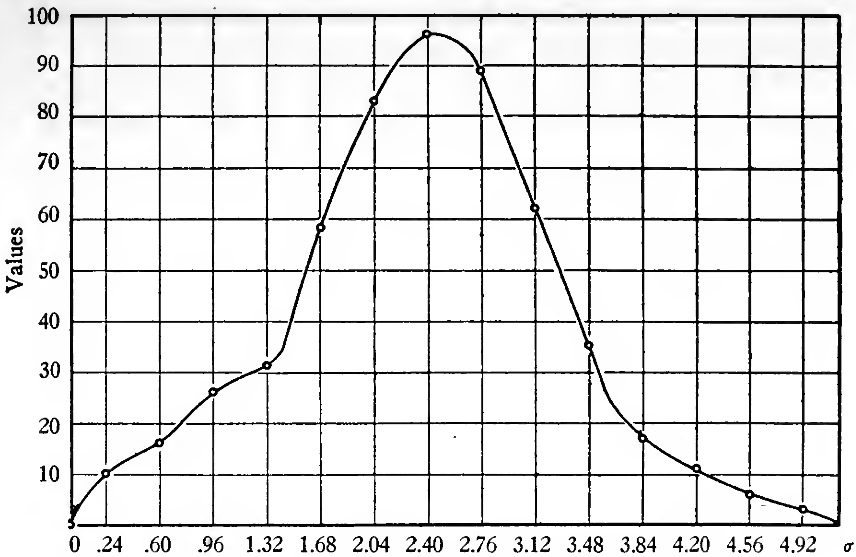

Fig. VI. Curve of 575 values of the binaural threshold of time-perception (data smoothed twice)

the values ranged from .12 σ to 4.80 σ with the average at 2.69 σ. In case of the weakest absolute intensities, the values ranged from .12 σ to 4.68 σ with the average at 2.70 σ. Calculations in both of these cases were based upon 86 judgments. Furthermore, the judgments by individual *O*s, in most cases, were distributed over the entire range represented in Table III.

The *O*s described the binaural sound, as it approached the dichotic threshold, as a double sound, or as a single sound with two peaks of intensity. Also, they reported illusory movements of the sounds from one ear to the other just before the fused sound divided into dichotic sounds. Four of the *O*s occasionally

reported that the fused sound divided before it came to the aural axis, and that the dichotic sounds were localized toward the median plane instead of appearing at the aural axis.

It is found therefore that the fused sound divides into dichotic sounds when the temporal interval is 2.36 ± 98 σ.

When the temporal interval ranges around this value, called the dichotic threshold or normal binaural time-threshold, illusory movements of sound and double sounds on the side of the ear which receives the prior stimulus are perceived.

General summary. As was seen in the introductory section, Klemm defines the threshold of localization as 2 σσ. Von Hornbostel and Wertheimer, Hecht, and Wittmann place the value at 30 σσ; while Bennett holds that direction can not be exactly perceived until the temporal interval is as large as 1 σ. But the experiments reported in this section indicate that the threshold of localization may be defined as any value ranging from .06 σ, where the binaural sound is localized approximately ten per cent of the time as being just outside of the mid-region, to .48 σ above which "rightness" and "leftness" can be determined with certainty.

Localization is toward the side of the ear which receives the prior sound when the interval ranges above the threshold of localization. This is in agreement with the findings reviewed in the introduction.

Von Hornbostel and Wertheimer, and Hecht hold that the fused sound comes to the aural axis when the temporal interval is .63 σ; while Wittmann states that it comes to the aural axis when the interval is 2.6 σ. According to this investigation, the value is approximately 2 σ, the value which Klemm assigns to this point.

The binaural sound normally divides into dichotic sounds when the interval is of the order of 2.36 ± .98 σ. This may be called the normal binaural threshold of time-perception. Klemm names 2 σ as the value. Banister places the point at 1.75 σ, while Wittmann holds that the point is 16.5 σ.

The binaural sound gradually changes in pitch and loudness

from low and loud in the median plane to higher in pitch and less loud at the lateral positions. It varies, also, in its volumic and durational aspects, being smaller and more brief in duration when it is in the median plane. These statements are in agreement with those of Wittmann. Klemm agrees, except that he holds that the pitch of the sound changes in reverse order. According to Banister the sound varies in intensity and volume, but not in pitch.

Linear relationship obtains between angular displacement and time-difference. This agrees in general with the statements of Wittmann, and von Hornbostel and Wertheimer.

The course of the binaural sound is more or less curved, corresponding approximately to a semicircle consisting of a 90° arc on either side of the median plane.

Other Temporal Aspects

In an effort to set forth more completely the temporal aspects of binaural sound localization, *i.e.*, to name other temporal aspects, an additional series of experiments was performed. These experiments dealth with the following problems which arose in the course of the investigation:

A. The behavior and characteristics of the dichotic sounds,
   1. Below the normal dichotic threshold; or, the second moving phantom sound;
   2. Above the normal binaural threshold of time-perception; or, the diotic threshold;

B. Other auditory illusions,
   1. The double sounds at both ears;
   2. The third moving phantom sound.

The dichotic sounds were partially investigated by Klemm and by Banister, as was seen in the historical survey in the first section of this report; but these investigators did not determine the point in the temporal series at which the dichotic sounds become qualitatively and intensively alike, or diotic. The double sounds and the two extra phantom sounds were discovered, by chance, in the course of the present investigation.

The Dichotic Sounds. In order to determine the behavior, or courses, and characteristics of the dichotic sounds two experiments covering different ranges of the temporal series were performed.

The Second Moving Phantom. The first [5] of these experiments dealt with the dichotic sounds below what has been called the normal binaural threshold of time-perception. The purpose was to determine whether, as was suggested by the reports of the *O*s in the last experiment of the foregoing survey, the sound that appears on the side opposite the fused sound is localized consistently as a moving phantom sound. The set-up was the same as that for the first two of the previous experiments. The *O*s were instructed to record the angular displacement of both sounds when two sounds were heard. The temporal series ranged in steps of .12 σ from O to 3 σ.

Table IV presents the averages of approximately twenty estimates by three different *O*s, of the angular displacement at the different points in the temporal series, of the sound that appears on the side opposite the fused sound.

This table shows that two of the *O*s consistently localized the sound opposite the fused sound as a moving phantom sound which was perceived throughout the entire range of the temporal series; and that the third *O* consistently localized the sound as a moving phantom source when the series ranged around the normal dichotic threshold. The other *O*s, *Hd* and *Ha,* who took part in this experiment, often localized the sound at 75° from the median plane as the interval ranged around the normal binaural time-threshold.

The *O*s described the second moving phantom sound as being much weaker, or fainter, and higher in pitch, throughout the series, than the fused sound. It was described as changing in intensity from very weak near the median plane to stronger at the aural axis.

The results therefore indicate that a second phantom sound

[5] This experiment was performed after the other experiments in this series; but, for the sake of continuity, it is placed first in this report.

is localized opposite the fused sound throughout the temporal series from the localization-threshold to the normal binaural time-threshold.

This phantom sound is much weaker than the first binaural sound when they are both localized near the median plane. As

TABLE IV. *Showing how the second phantom sound was localized*

Time-difference	Observers		
σ	*L*	*Ml*	*H*
000	....*	....	....
.12	....	1**	....
.24	....	3**	....
.36	....	7± 3	....
.48	6± 1	20± 8	....
.60	6± 1	23±11	....
.72	6± 1	35±16	....
.84	8± 2	48±22	....
.96	8± 2	55±23	....
1.08	9± 3	69±23	....
1.20	12± 5	74±19	....
1.32	15± 5	80±16	....
1.44	18± 9	84±10	....
1.56	22±11	90± 0	....
1.68	28±11	90± 0	....
1.80	35±12	90± 0	....
1.92	42±13	90± 0	....
2.04	50±12	90± 0	....
2.16	56±14	90± 0	28±11
2.28	62±10	90± 0	61±13
2.40	68± 9	....	63±15
2.52	70±11	....	74±10
2.64	75± 6	....	86± 5
2.76	79± 7	....	83±12
2.88	80± 4	....	82±13
3.00	81± 4	....	90± 0

* No estimates were given at these points.
** Each of these numbers represents an estimated, not a true average, because of errors, hence no mean deviation could be found.

they approach the aural axis, this condition is somewhat altered, with the second sound still noticeably weaker than the first.

The diotic threshold. The second experiment in this series was concerned with the dichotic sounds above the normal binaural threshold of time-perception. The aim was twofold: *viz.* (1) to find the point in the temporal series at which the dichotic sounds become qualitatively alike and equally intense, or diotic; and (2) to determine if the sounds are localized consistently at

stationary positions on the aural axis. The sound sources were adjusted to 72 cm., respectively, from the ears. The *Os* were instructed to record variations in both angular displacement and distance, and to indicate the point in the series at which the sounds appeared to be alike in quality, and equally loud. The temporal series ranged from 0 to 20.40 σ, in steps of .68 σ.

A summary of the judgments concerning the point in the temporal series at which the dichotic sounds become diotic is presented in Table V. The values are grouped around the mid-points of class intervals, in steps of 2.04 σ.

The average value assigned as the diotic threshold was found from the data in Table V to be 9.48 ± 3.5 σ. The individual distributions shown in the table indicate the consistency of the judgments of the individual *Os*.

TABLE V. *Distribution of forty-eight judgments of the value of the diotic threshold*

σ, Time difference	Observers				
	Ha	*Ho*	*S*	H	All
4.76	1	—	1	—	2
6.80	1	2	2	3	8
8.84	4	4	2	1	11
10.88	3	3	1	4	11
12.92	2	5	1	1	9
14.96	1	2	—	—	3
17.00	1	—	—	1	2
19.04	—	—	1	1	2

The dichotic sounds were consistently localized at 90°; but they were described by the *Os* as shifting in position along the aural axis, moving slightly away from the ears as the temporal interval was increased; and toward the ears when the interval was decreased. The *Os* reported that the sound on the side of the prior stimulus sound became weaker and higher in pitch while the sound on the other side became louder and lower in pitch, as the temporal interval was increased, until they were perceived to be alike in quality, and equally loud, at the diotic threshold.

When the temporal interval ranged above the diotic threshold the following auditory phenomena were consistently reported by one or more of the *Os*. (1) Illusory movements were reported

by *H*, who described the phenomena variously as "lines of movement," "strings of sound," "flashes of sound," "sounds of long duration." (2) "Double sounds," or "split sounds," or "sounds with two peaks of intensity," instead of the whole sound on the side of the prior stimulus sound, were consistently reported by *Ho* and *Ha*. These *O*s also occasionally reported double sounds at both sides. (3) A third sound that "moves gradually away from the median plane and back" was reported by *S* and *H*.

It is found, then, that the diotic threshold, or the point in the temporal series at which the dichotic sounds become qualitatively alike and equally intense, is around $9.48 \pm 3.5\ \sigma$.

The dichotic sounds are consistently localized at 90°, but they shift in position along the aural axis, moving away from the ears as the temporal interval is increased, and toward the ears when the interval is decreased.

Other auditory phenomena appear as the interval extends above the diotic threshold.

Other Auditory Illusions

The "double sounds" and the third moving phantom sound, which were reported in the above experiment, were further investigated in the last two experiments of this series. Because of the complexity of the situation in both cases, study of these phenomena proved difficult. The third moving phantom sound was so weak in comparison with the other sounds heard at the same time, and so elusive, that the *O*s had difficulty in attending to it. In the case of "double" sounds, attention had to be divided between two pairs of sounds, one pair at each ear. The paired sounds appeared so close together in time that the *O*s had difficulty in attending to all of the sounds at the same time. The element of fatigue could not be avoided because thorough investigation of the phenomena in both cases required very long temporal series, which if presented in segments would have caused shifts of attention on the part of the *O*s.

The double sounds. The experiment which dealt with the double sounds was a continuation of the foregoing experiment.

The two *Os*, *Ho* and *Ha*, who reported the phenomena in the above experiment, were instructed to record the direction and the number of sounds they heard at each point in the series. They were asked to attend also to the complete situation, and to report all of their auditory experiences. The series ranged from 0 to 44.2 σ, in steps of .68 σ.

A summary of the *Os*' reports concerning the number and the direction of the sounds heard at each point in the series is presented in Table VI. This table shows, in percentages based upon

TABLE VI. *Frequency distribution of reports of paired sounds at both ears (by percentages)*

	Os					*Os*			
Time-difference	*Ha* p*	*Ha* 1	*Ho* p	*Ho* 1	Time-difference	*Ha* p	*Ha* 1	*Ho* p	*Ho* 1
σ	%	%	%	%	σ	%	%	%	%
4.08	40	10	—	—	18.36	80	60	100	10
4.76	40	10	—	—	19.04	70	80	70	10
5.44	40	10	—	—	19.72	60	50	60	20
6.12	20	—	10	—	20.40	80	70	70	30
6.80	60	—	10	—	21.08	70	60	60	30
7.48	70	10	10	—	21.76	80	30	60	30
8.16	60	30	30	—	22.44	60	40	70	20
8.84	70	40	50	—	23.12	50	80	70	20
9.52	80	20	60	—	23.80	60	90	70	20
10.20	90	—	60	—	24.48	60	70	40	10
10.88	70	40	60	—	25.16	60	70	40	10
11.56	60	30	60	—	25.84	60	70	30	—
12.24	60	50	70	—	26.52	50	70	20	—
12.92	80	60	80	—	27.20	60	90	10	—
13.60	70	40	80	—	27.88	60	90	20	—
14.28	60	50	80	—	28.56	70	90	10	—
14.96	60	60	90	—	29.24	70	90	10	10
15.64	30	60	90	—	29.92	70	70	10	10
16.32	50	70	90	10	30.60	20	70	10	10
17.00	70	90	90	10	——	—	—	—	—
17.68	50	80	90	10	——	—	—	—	—

* p indicates that localization was toward the side of the prior sound.
1 indicates that localization was toward the side of the later sound.

ten reports at each point in the series, how frequently paired sounds were reported at either ear.

Table VI shows that paired sounds were heard over a greater range of the temporal series on the side of the ear receiving the prior sound than they were on the other side; and that paired sounds were more consistently reported on the side of the prior

sound. This table does not show the distribution over the entire range of time-differences covered in the experiment, because only a few of the series extended beyond the range represented in this table. *Ho* reported that the paired sounds "dropped out" when the series ranged above 39.44 σ; but *Ha* reported paired sounds over the entire range represented in this experiment.

Both of the *O*s reported "a sound localized toward the median plane." *Ha* described it as moving toward the median plane and back, as the interval was increased. *Ho* described the sound as "too elusive for me to keep up with." *Ha* frequently reported illusory movement, "long sounds" on the side of the ear receiving the prior sound.

Paired sounds are therefore perceived at both ears when the temporal interval ranges somewhat above the diotic threshold.

The third phantom sound. In the experiment which was concerned with the third moving phantom sound the *O*s, *S* and *H,* who reported this sound in the course of the second experiment of this series, were instructed to record the direction and the angular displacement of the sound from the median plane, and to study the course of the sound as the series were presented. Otherwise the conditions for this experiment were the same as those for the above experiment.

Table VII presents the angular displacement according to the estimates of *S,* of the third phantom sound at the different points in the temporal series up to 23.12 σ. The sound was still reported by this *O* above this point, but it was not so consistently localized.

In an occasional series *H* reported the angular displacement of the third phantom fairly consistently, the estimates agreeing in general with those of *S*. *H* generally reported illusory movements of sound, "lines of sound" which varied in length. These lines most often crossed through the median plane, the longer end being on the side of the prior stimulus sound.

The *O*s described the course of the sound as a more or less straight line extending from the median plane to the aural axis. The sound moved from the median plane to the aural axis on the

side of the prior sound as the interval ranged from approximately 3 σ to 10 σ. Further increase of the interval brought the phantom sound back to the median plane when the interval ranged around 20 σ. When the interval was increased above this point, *H* reported illusory movements of sound only, in addition to the

TABLE VII. *Showing how S localized the third phantom sound*

Time-difference	Series							
	ascending				descending			
σ	1	2	3	4	1	2	3	4
3.40	—*	—	0	—	—	—	5	—
4.08	—	—	10	—	—	—	10	—
4.76	5	—	15	5	—	—	30	—
5.44	10	10	20	10	—	—	30	5
6.12	15	15	25	15	—	—	40	10
6.80	25	25	30	25	—	—	45	15
7.48	35	35	45	30	—	—	50	30
8.16	45	45	50	45	—	—	60	45
8.84	55	50	60	50	—	90	75	60
9.52	60	60	70	60	90	—	80	75
10.20	75	75	80	75	90	90	90	90
10.88	80	80	85	85	90	—	90	80
11.56	90	85	90	90	90	90	75	60
12.24	90	90	85	90	90	90	60	45
12.92	90	90	75	80	90	85	50	30
13.60	80	90	70	75	85	80	45	10
14.28	70	80	65	60	80	75	30	5
14.96	55	75	60	50	75	65	25	0
15.64	45	70	50	45	70	55	15	**
16.32	35	65	35	35	60	45	10	**
17.00	20	60	25	25	55	35	5	**
17.68	15	50	15	15	50	25	0	**
18.36	10	45	10	8	45	20	**	**
19.04	4	30	7	5	35	15	**	**
19.72	2	20	0	2	25	8	**	**
20.40	**	15	**	**	15	5	**	**
21.08	**	10	**	**	10	0	**	**
21.76	**	4	**	**	5	**	**	**
22.44	**	0	**	**	0	**	**	**
23.12	**	**	**	**	**	**	**	**

* No estimate given.
** Localization was toward the side of the ear receiving the later stimulus.

stimulus sounds. According to *S*, the third phantom swung over to the side of the later stimulus sound and described a course similar to that described on the side of the prior stimulus sound, as the interval was increased above 20 σ.

A third phantom sound is localized, therefore, when the temporal interval ranges above the diotic threshold.

Illusory movements of sound are generally perceived when the temporal interval ranges above the diotic threshold.

General summary. A phantom, or binaural sound, heretofore undiscovered, is localized toward the side opposite the already established binaural or fused sound as the temporal interval ranges from the localization-threshold to the binaural time-threshold.

The diotic threshold, or the point in the temporal series at which the dichotic sounds become qualitatively alike and equally intense, is around $9.48 \pm 3.5\ \sigma$. As was seen in the historical summary in the introductory section, Banister reports that the dichotic sounds finally become equally intense as the temporal interval is increased above the dichotic threshold; but he does not indicate at what point.

The dichotic sounds are consistently localized at 90°; but they shift in position along the aural axis, moving slightly away from the ears when the temporal interval is increased, and toward the ears when the interval is decreased.

Paired sounds are perceived at both ears when the temporal interval ranges somewhat above the diotic threshold. These phenomena were discovered in the course of this investigation.

A third phantom sound is localized when the interval ranges above the diotic threshold. This sound also was discovered during the course of this investigation.

Localization with Changes in Absolute Intensity

In the course of the foregoing experimental survey of known temporal aspects of binaural sound localization, the following questions arose: Is the present investigation dealing with a special case of localization since the stimulus sounds remain the same in absolute intensity? If the previous investigators had employed stimulus sounds of the same absolute intensity would their values have been discrepant? Does localization on the basis of the temporal factor depend upon the absolute intensity of the stimulus sounds?

In an attempt to answer these questions five different experi-

ments covering the same range of the temporal series, but employing stimulus sounds of different absolute intensity, were performed. The absolute intensities were determined by adjusting the sound sources to different, equal distances from the ears in each case. In the first experiment the sound sources were

TABLE VIII. *Showing localization by L in the different situations of changed absolute intensity*

Time-difference σ	a	b	c	d	e
000	X*	X*	X*	X*	X*
.12	4**	4**	2**	8**	4**
.24	8**	8**	6**	10**	8**
.36	18± 8	14± 5	12± 4	22± 8	14± 6
.48	23± 8	18± 4	17± 4	28±10	19± 7
.60	28± 9	23± 5	22± 4	32±10	23± 9
.72	32±10	26± 6	26± 5	37±11	27± 8
.84	36± 9	31± 6	30± 5	42±11	32± 7
.96	41± 9	35± 6	34± 5	44±11	38± 8
1.08	45± 9	40± 5	38± 4	47±10	41± 8
1.20	50± 9	44± 6	42± 4	49±11	45± 7
1.32	54± 9	49± 7	45± 4	52±10	48± 7
1.44	59± 8	53± 8	47± 5	55± 9	52±10
1.56	62± 8	58± 9	53± 7	59±10	58± 8
1.68	66± 8	63± 8	61± 7	62± 8	60± 8
1.80	70± 6	67± 8	66± 6	65± 9	62± 7
1.92	71± 7	72± 7	69± 6	68± 9	64± 7
2.04	73± 6	75± 6	72± 6	69± 7	66± 7
2.16	76± 5	83± 6	77± 4	72± 7	68± 6
2.28	78± 6	78± 6	79± 4	75± 5	69± 6
2.40	81± 6	79± 5	81± 5	75± 4	71± 6
2.52	83± 6	81± 6	83± 5	78± 5	72± 5
2.64	83± 5	82± 6	85± 4	80± 5	73± 5
2.76	85± 4	82± 6	86± 5	81± 5	73± 4
2.88	82± 5	81± 6	86± 6	83± 4	74± 3
3.00	83± 6	80± 3	— —	84± 2	75± 2

Legend same as in Table II.

adjusted to 6 cm., and the absolute intensity in this case was called *a*. In the second, third, fourth, and fifth experiments the sound sources were adjusted to 18 cm., 36 cm., 72 cm., and 108 cm., respectively; and the corresponding absolute intensities were called *b, c, d,* and *e*. The same *O*s were used in all of the experiments of this series. They were instructed to record the direction and the angular displacement of the localization at the different points in the temporal series. The temporal interval ranged from 0 to 3 σ in steps of .12 σ.

Tables VIII, IX, X, XI, and XII summarize the data obtained from this series of experiments. Each of these tables presents the localization by a single *O* in the five different situations of changed absolute intensity.

The tables give the averages, with average deviations, of approximately twenty estimates of angular displacement at the

TABLE *IX*. *Showing localization by Ml in the different situations of changed absolute intensity*

Time-difference σ	*a*	*b*	*c*	*d*	*e*
000	X*	X*	X*	X*	X*
.12	5**	5**	4**	5**	6**
.24	11**	12**	9**	14**	11**
.36	20± 6	18±11	19± 9	18±10	21±14
.48	29±14	31±14	28±11	32±16	36±17
.60	43±17	42±16	40±14	45±19	48±23
.72	56±19	57±16	51±18	58±16	62±24
.84	72± 8	66±15	62±19	76± 9	72±20
.96	79± 7	78±11	70±18	83± 7	78±14
1.08	82± 6	83± 8	73±19	87± 4	81±20
1.20	83± 6	86± 6	79±16	89± 1	84± 7
1.32	82± 6	86± 5	82±11	89± 1	86± 6
1.44	85± 4	88± 3	83±11	89± 2	88± 5
1.56	88± 2	88± 3	89± 1	88± 3	88± 3
1.68	89± 1	89± 2	89± 1	89± 1	89± 2
1.80	90± 0	89± 2	89± 1	89± 2	90± 0
1.92	90± 0	90± 0	90± 0	89± 1	90± 0
2.04	90± 0	90± 0	89± 2	90± 0	90± 0
2.16	90± 0	90± 0	90± 0	90± 0	90± 0
2.28	90± 0	90± 0	89± 2	90± 0	90± 0
2.40	90± 0	90± 0	89± 2	90± 0	90± 0
2.52	90± 0	90± 0	89± 2	90± 0	90± 0
2.64	90± 0	90± 0	90± 0	90± 0	90± 0
2.76	90± 0	90± 0	90± 0	90± 0	90± 0
2.88	90± 0	90± 0	90± 0	90± 0	90± 0
3.00	90± 0	90± 0	90± 0	90± 0	90± 0

Legend same as in Table II.

different points in the temporal series. The data given for absolute intensity *d* in these tables were presented in slightly different form in Table II.

The tables show that the *O*s were fairly consistent in their estimation of the angular displacement of the fused sound from the median plane in the different situations. The inconsistencies that are apparent do not indicate, from a comparison of the

records of all the *O*s, that one absolute intensity is more effective than another.

All of the *O*s reported that localization was easier when the sound sources were near the ears than it was when the sources were far away, but according to the data in the tables, the *O*s in general were just about as consistent in their estimation of angular displacement in situation *e* as in situation *a*. As was seen in a previous section, the normal dichotic threshold does not depend upon the absolute intensity of the stimulus sounds.

TABLE X. *Showing localization by Ha in the different situations of changed absolute intensity*

Time-difference σ	*a*	*b*	*c*	*d*	*e*
000	X*	X*	X*	X*	X*
.12	6**	8**	12**	13**	6**
.24	15**	18**	19**	27**	12**
.36	28±12	35±17	36±17	46±17	17±10
.48	40±13	44±17	46±18	43±18	18± 6
.60	51±18	52±15	52±13	50±26	27±10
.72	60±14	60±11	59±17	60±21	38±17
.84	68±16	66±10	64±17	65±18	45±17
.96	78± 9	71±11	73±15	72±17	57±18
1.08	78±12	73±11	74±14	77±12	62±17
1.20	75±12	75± 9	76±14	78± 7	64±12
1.32	81±11	82± 9	75±12	80± 9	71±13
1.44	81± 7	81±10	78± 9	83± 9	79±10
1.56	85± 7	82±10	82± 8	88± 3	79±10
1.68	88± 3	80± 8	84± 8	87± 3	82±10
1.80	88± 4	83± 8	86± 6	88± 3	87± 4
1.92	89± 2	83± 8	86± 6	88± 3	88± 3
2.04	89± 2	86± 6	87± 5	89± 1	87± 4
2.16	89± 2	84± 6	85± 6	89± 1	87± 5
2.28	88± 3	85± 6	86± 6	90± 0	87± 4
2.40	88± 3	88± 3	86± 5	89± 1	88± 4
2.52	90± 0	88± 3	87± 4	89± 1	88± 3
2.64	90± 0	87± 4	88± 3	89± 1	89± 1
2.76	90± 0	88± 3	88± 3	88± 1	89± 1
2.88	90± 0	89± 1	89± 1	88± 3	90± 0
3.00	90± 0	89± 1	89± 1	88± 1	90± 0

Legend same as in Table II.

According to the reports of the *O*s the only difference between the course of the fused sound when the sources were close to the ears and that when the sources were farther away was one of spatial distance, the course lying close to the head when the

stimulus sounds were close to the ears, and farther away from the head when the stimulus sounds were farther away.

The second phantom sound was consistently reported in all of the situations by *L* and *M*.

The conclusion follows that localization on the basis of the temporal factor does not depend upon the absolute intensity of

TABLE XI. *Showing localization by H in the different situations of changed absolute intensity*

Time-difference σ	*a*	*b*	*c*	*d*	*e*
000	X*	X*	X*	X*	X*
.12	11**	13**	6**	3**	6**
.24	22**	18**	14**	10**	14**
.35	43±20	31±17	24±14	21±15	23±14
.48	58±18	47±14	38±15	29±17	33±20
.60	68±14	59±13	46±14	43±20	43±17
.72	77±11	70±13	57±19	47±23	50±14
.84	78±11	77±11	65±17	53±24	61±17
.96	82±10	82± 7	67±16	65±23	67±17
1.08	85± 6	84± 7	76±13	68±22	70±13
1.20	84± 7	87± 4	80±11	73±22	74±12
1.32	79± 9	88± 2	84± 8	80±12	77±11
1.44	83± 6	89± 1	85± 7	84± 9	79± 9
1.56	84± 5	88± 2	87± 4	85± 8	85± 5
1.68	89± 1	89± 1	89± 1	88± 3	86± 4
1.80	89± 1	89± 1	89± 1	89± 1	88± 2
1.92	90± 0	90± 0	90± 0	89± 1	88± 2
2.04	89± 1	90± 0	90± 0	89± 1	89± 1
2.16	89± 1	90± 0	90± 0	90± 0	89± 1
2.28	90± 0	90± 0	90± 0	90± 0	89± 1
2.40	90± 0	90± 0	90± 0	90± 0	89± 1
2.52	90± 0	90± 0	90± 0	90± 0	89± 1
2.64	90± 0	90± 0	90± 0	90± 0	90± 0
2.76	90± 0	90± 0	90± 0	90± 0	90± 0
2.88	90± 0	90± 0	90± 0	90± 0	90± 0
3.00	90± 0	90± 0	90± 0	90± 0	90± 0

Legend same as in Table II.

the stimulus sounds, when absolute intensity-differences range between comparatively weak and comparatively strong absolute intensities.

It appears, therefore, that the differences in the values assigned by the previous investigators to the critical points in the temporal series can not be accounted for on the basis of differences in the absolute intensities employed.

TABLE XII. *Showing localization by Hd in the different situations of changed absolute intensity*

Time-difference σ	a	b	c	d	e
000	X*	X*	X*	X*	X*
.12	8**	18**	10**	15**	9**
.24	18**	26**	18**	24**	20**
.36	33±11	40±22	33±12	31±13	29± 8
.48	47± 8	44±13	46±15	39±11	32±11
.60	55± 7	53±15	48±18	45±12	38±15
.72	62±11	58±11	55±12	53±17	42±16
.84	65±10	73±10	60±13	55±14	45±12
.96	69± 9	70± 9	61±15	54±15	50±14
1.08	73± 8	71±10	69±14	58±14	53±14
1.20	75±10	74± 8	68±13	62±14	55± 9
1.32	76±11	81± 7	71±17	64±14	59± 8
1.44	80± 9	84±10	72±14	68±11	64± 8
1.56	81± 9	85± 6	75±15	71±13	64± 8
1.68	82±10	88± 4	78±12	72±10	66± 5
1.80	85± 8	89± 3	82±10	72±12	68± 8
1.92	85± 8	90± 2	83± 9	72±12	74± 8
2.04	87± 4	90± 1	86± 6	72±10	74± 5
2.16	89± 2	90± 2	87± 6	74± 9	74± 6
2.28	90± 0	90± 2	86± 5	73± 9	77± 5
2.40	90± 0	90± 3	86± 5	74± 9	77± 3
2.52	90± 0	90± 3	86± 5	74± 8	78± 4
2.64	90± 0	90± 5	87± 4	74± 8	78± 3
2.76	90± 0	90± 6	85± 5	76± 7	80± 2
2.88	90± 0	93± 4	86± 4	74± 7	80± 2
3.00	90± 0	93± 4	90± 0	74± 7	80± 1

Legend same as in Table II.

The Influence of Sound Shadows on Localization

The data obtained from the foregoing experiment in which the localization of the second phantom sound was studied indicate that the diffracted stimulus sounds may perhaps be effective in localization. If this be the case, intensity-differences due to interference, and perhaps phase-differences due to the arrival of successive wave fronts at the ears, may be effective along with time-differences when localization is studied with a single impulse technique.

If phase-differences and intensive-differences be effective along with temporal-differences in determining localization, a shadow larger than that cast by the head, because it would reduce the intensity of the diffracted sounds and alter the phase relationship of the successive stimuli, should affect the localization. An

experiment which dealt with sound shadows in localization was performed, in an attempt to determine whether localization is due entirely to the temporal factor when two equally intense sounds that are qualitatively alike are presented, one at each ear, with a slight temporal interval.

The set-up for this experiment was the same as that for the previous experiments in which angular displacement was studied, except that a median plane partition one meter in diameter was so attached in the system that the aural axis passed through the center, and perpendicular to the plane. The sound sources were adjusted to a distance of 72 cm., respectively, from the ears. Four of the *O*s who took part in the foregoing experiments were used in this experiment, thus making direct comparison of the localization by the individual *O*s in the different situations possible. The *O*s were instructed to indicate the direction and the angular displacement of the sound heard as the temporal series were run. The temporal interval ranged, in steps of .12 σ, from 0 to 3 σ.

Table XIII presents a summary of the data obtained from this experiment. The table gives the averages, with average deviations, of approximately twelve estimates, by the individual *O*s, of the angular displacement of the fused sound from the median plane, at each point in the temporal series.

A comparison of the data presented in Table XIII with the data presented in Tables II, VIII, IX, XI, and XII shows that there are no significant differences in the localization by the individual *O*s in the different situations. The individual differences which appear in one situation appear in all the situations. For the individual *O*s the fused sound comes to the maximal lateral position in approximately the same order, *i.e.*, the same relationship obtains between angular displacement and time-difference, according to the individual *O*s, in the different situations.

The average temporal interval at which the *O*s reported that the fused sound divided in this situation was 2.36 σ, which is slightly less than the average interval at which the fused sound divides when the situation is normal.

L consistently reported the second phantom sound, which he described as being " very weak and hard to keep up with," in this situation. *Ml* and *H* reported this phantom in only two of the twelve series that were run.

It follows, then, that the second phantom sound tends to disappear when a sound shadow larger than that cast by the head is introduced.

A sound shadow larger than that cast by the head does not affect the localization of the first phantom sound, nor the interval at which it divides into dichotic sounds. It appears, therefore, that time-difference is the effective agent in determining localization.

TABLE XIII. *Showing localization when a sound shadow larger than that cast by the head is introduced*

Time-difference	Observers			
σ	*L*	*Ml*	*H*	*Hd*
000	X*	X*	X*	X*
.12	3**	5**	7**	5**
.24	7**	9**	14**	14**
.36	11± 5	17± 9	24±13	24±14
.48	14± 5	28±15	37±17	32±14
.60	17± 5	38±23	49±16	37±16
.72	23± 6	48±21	58±17	43±17
.84	23± 3	61±18	69±16	40±20
.96	27± 5	66±16	74±16	50±18
1.08	29± 6	72±19	78±11	54±14
1.20	33± 5	77±14	81± 9	56±14
1.32	38± 6	88± 9	83± 6	59±12
1.44	42± 7	87±10	86± 5	63±11
1.56	47± 7	83±11	87± 5	68±11
1.68	50± 7	85± 8	89± 2	73±10
1.80	54± 7	87± 4	90± 0	78± 8
1.92	60± 6	90± 0	90± 0	80± 9
2.04	65± 8	90± 0	90± 0	81± 9
2.16	71± 5	90± 0	90± 0	82± 7
2.28	76± 5	90± 0	90± 0	83± 7
2.40	78± 5	90± 0	90± 0	85± 4
2.52	83± 4	90± 0	90± 0	85± 5
2.64	86± 2	— —	90± 0	83± 6
2.76	89± 2	— —	90± 0	84± 5
2.88	90± 0	— —	90± 0	85± 4
3.00	90± 0	— —	90± 0	87± 2

Legend same as in Table II.

Experiments to Check the Technique

A single impulse technique presupposes equality of the stimuli in both intensity and quality. Since the technique used in this investigation up to this point employed sounds that were set up by electric sparks which may be considered as more or less variable, it was decided that some measure of the equality of the stimuli should be found. No satisfactory physical means of measuring the intensity and the wave-form of the sounds was available. It was necessary to find other methods of showing whether the stimuli were equally effective in determining localization. The problem was attacked from three different angles, in a series of experiments.

Localization Toward the Right and Toward the Left

The first experiment of the series was concerned with localization toward the right and toward the left. The aim was to determine whether localization was distorted toward either side, and to find if localization toward one side was more consistent than localization toward the other. For this experiment, the sound sources were adjusted to 72 cm. from the ears. The *O*s were instructed to record the direction and the angular displacement of the fused sound from the median plane at each point in the temporal series. The temporal interval ranged from 0 to 2.76 σ, in steps of .12 σ.

Table XIV shows the results. Many individual differences are evident. *L* and *Hd* are more consistent in localization toward the left. *Ml* is more consistent in localization toward the right. When the temporal interval is relatively short, *H* is more consistent toward the right; but when the interval ranges above .72 σ he localizes more consistently toward the left. *Ha* localizes as consistently toward one side as toward the other. Localization is slightly distorted toward the right for *L;* but it is distorted in the other direction for *Ml* and *H*. The ratio between angular displacement and temporal difference is approximately equal in localization toward the right and toward the left for *Ha* and *Hd*.

TABLE XIV. *Showing localization toward the right and toward the left*

Time-difference	Observers									
	L		*Ml*		*Ha*		*H*		*Hd*	
σ	Rt.	Lt.	Rt.	Lt.	Rt.	Lt.	Rt.	Lt.	Rt.	Lt.
000	X*	X*	X*	X*	X*	X*	X*	X*	X*	X*
.12	12**	4**	2**	8**	4**	18**	2**	6**	21**	8**
.24	19**	11**	7**	22**	16**	31**	6**	10**	25**	23**
.36	25±11	18± 5	14± 8	23±12	28±14	39±14	8± 5	26±18	33±15	30± 8
.48	35±13	22± 5	25±11	40±18	44±21	48±21	19±11	41±18	40±16	44± 8
.60	40±14	25± 4	37±11	46±23	56±23	53±25	25±12	60±18	46±19	52± 6
.72	45±16	30± 4	59±11	60±18	55±17	61±24	31±19	61±18	47±22	63± 8
.84	47±14	34± 5	75± 8	78±10	64±15	68±20	38±20	67±14	50±17	71± 8
.96	50±14	38± 5	83± 7	84± 7	74±14	72±16	58±18	76±13	52±18	59±10
1.08	52±14	42± 5	87± 4	90± 2	82±10	75±12	62±24	82± 9	61±17	64±12
1.20	53±15	45± 5	89± 1	89± 1	82±10	79± 9	67±13	86± 7	60±16	67±10
1.32	56±12	50± 5	88± 3	90± 0	83± 8	82± 7	76±14	83±10	61±14	68±11
1.44	56±11	55± 7	88± 3	90± 0	85± 7	85± 6	82±12	85± 7	60±16	73±10
1.56	61±11	57± 8	87± 4	90± 0	89± 2	87± 4	82± 9	85± 8	69±16	74± 8
1.68	63±11	62± 6	89± 1	90± 0	90± 0	85± 7	88± 2	90± 0	69±16	76± 6
1.80	66±10	65± 6	88± 3	90± 0	90± 0	87± 5	89± 1	90± 0	66±17	78± 3
1.92	68±10	67± 5	89± 1	90± 0	90± 0	86± 5	90± 0	90± 0	67±16	78± 3
2.04	70± 9	68± 6	90± 0	90± 0	90± 0	90± 0	90± 0	90± 0	68±14	77± 5
2.16	72± 7	72± 5	90± 0	90± 0	90± 0	90± 0	90± 0	90± 0	71±12	77± 4
2.28	75± 7	75± 3	90± 0	90± 0	90± 0	89± 2	90± 0	90± 0	72±10	76± 4
2.40	77± 6	76± 3	90± 0	90± 0	90± 0	90± 0	90± 0	90± 0	73±10	78± 5
2.52	78± 5	77± 3	90± 0	90± 0	90± 0	89± 2	90± 0	90± 0	73±10	75
2.64	78± 6	82± 3	90± 0	90± 0	90± 0	89± 1	90± 0	90± 0	73±10	75
2.76	78± 6	83± 2	90± 0	90± 0	90± 0	90± 0	90± 0	90± 0	72± 7	75

Legend same as in Table II.

There are individual differences in localization toward the right and toward the left; but the differences are so distributed that they can not be considered as due to differences between the stimuli.

Localization with the Sound Sources Interchanged

The second experiment of this series differed from the first in only one respect, *viz.*, the sound sources were employed interchangeably in order to eliminate differences in localization due to possible differences between the ears of the individual *O*s.

Table XV presents the data obtained from this experiment. The table shows only the angular displacement of the fused sound from the median plane. The Rt. and Lt. in the table refer to the sources of the stimuli, not to the direction of the localization.

Table XV shows no significant differences between localization when the right sound was the prior stimulus and localization when the left sound was the prior stimulus. The ratio between angular displacement and temporal difference in localization when the right sound is the prior stimulus and when the left sound is the prior stimulus is approximately equal for all of the *O*s. The *O*s are about as consistent in localization when the right sound is the prior stimulus as they are when the left sound is the prior stimulus.

There are, then, no significant differences between localization with the right sound as the prior stimulus and localization with the left sound as the prior stimulus.

Localization with the Telephone Receiver Technique

In the last experiment of this series telephone receivers were substituted for the spark plugs as sound sources. The aim was to compare localization with the technique employed in this investigation up to this point with localization with the telephone receiver technique, which has been most generally employed by previous investigators. It was necessary to adjust the receivers so that they generated high-pitched sounds, because with low-pitched sounds localization was indefinite and uncertain.

TABLE XV. *Showing localization when the sound sources were interchanged*

Time-difference	Observers									
	L		*Ml*		*Ha*		*H*		*Hd*	
σ	Rt.	Lt.	Rt.	Lt.	Rt.	Lt.	Rt.	Lt.	Rt.	Lt.
000	X*	X*	X*	X*	X*	X*	X*	X*	X*	X*
.12	2**	5**	6**	2**	13**	16**	5**	5**	15**	8*
.24	4**	6**	13**	6**	22**	23**	5**	12**	22**	14**
.36	5± 2	9± 3	19± 6	9± 3	28±15	30±28	16± 6	23±11	28± 7	22±13
.48	11± 3	14± 3	29± 8	18± 7	44±19	38±29	32±11	31±11	32± 8	27±12
.60	14± 4	18± 3	40±12	33±10	48±17	31±17	42± 9	40±14	36± 9	28±13
.72	17± 6	23± 3	53±15	40± 8	52±18	40±18	54±12	46±16	40±10	37± 8
.84	20± 5	26± 4	62±17	55±11	58±20	52±18	64±12	62±16	48± 9	39± 6
.96	23± 5	31± 4	68±15	62±12	63±12	58±18	72±10	59±15	48±10	39± 6
1.08	29± 6	36± 4	74±15	69±11	73± 8	57±17	82± 7	67±15	52± 9	44± 5
1.20	34± 6	42± 4	76±17	72±10	76± 6	65±10	84± 6	78±10	53±10	46± 4
1.32	41± 5	48± 4	81±13	77±13	75±13	72± 8	85± 4	74±14	52± 6	49± 4
1.44	44± 7	51± 4	85± 8	83± 9	71±19	75± 5	87± 4	83± 7	58± 8	52± 3
1.56	51± 8	56± 3	86± 7	84± 9	72± 4	78± 4	88± 3	82± 6	60± 6	53± 4
1.68	56± 8	61± 4	87± 4	84± 8	82± 7	82± 7	88± 3	87± 4	64± 7	55± 3
1.80	59± 7	67± 5	88± 4	88± 3	82± 7	82± 7	88± 3	88± 3	65± 5	61± 4
1.92	64± 6	71± 5	89± 2	90± 0	87± 4	80± 6	90± 0	89± 1	68± 4	62± 3
2.04	68± 5	76± 5	89± 1	90± 0	77± 8	80± 6	90± 0	90± 0	70± 3	68± 3
2.16	71± 5	79± 5	90± 0	90± 0	86± 7	87± 4	90± 0	90± 0	73± 3	68± 3
2.28	74± 4	82± 5	90± 0	90± 0	85± 7	82± 7	90± 0	90± 0	74± 4	68± 2
2.40	76± 4	85± 2	90± 0	90± 0	82± 7	82± 7	90± 0	90± 0	74± 4	68± 2
2.52	80± 4	86± 1	90± 0	90± 0	85± 6	87± 4	90± 0	90± 0	76± 4	68± 2
2.64	84± 3	88± 1	90± 0	90± 0	87± 4	90± 0	90± 0	90± 0	75± 4	67± 2
2.76	86± 3	90± 0	90± 0	90± 0	87± 4	87± 4	90± 0	90± 0	75± 4	68± 2

Legend same as in Table II.

Table XVI summarizes the data obtained from this experiment. The results show that the ratio between angular displacement and temporal difference is much less in localization with this technique than in localization with the previous technique. A comparison of the data shown in this table with the data shown in the foregoing tables indicates in general that localization with the telephone receiver technique is not any more definite than localization with the previous technique.

The ratio between angular displacement and temporal difference is less in localization with the telephone receiver technique than

TABLE XVI. *Showing localization with the telephone receiver technique*

Time-difference σ	Observers *L*	*Ml*	*H*	*Hd*	*Ha*
000	X*	X*	X*	X*	X*
.12	4**	4**	4**	10**	9**
.24	10**	9**	11**	16**	31**
.36	10± 1	17± 9	19± 5	24±12	45±15
.48	14± 2	23±10	27± 5	29±13	51±15
.60	14± 2	33±12	34±11	35±14	55±13
.72	16± 3	43±16	39±16	38±15	57±17
.84	17± 4	52±18	39±16	39±17	59±13
.96	18± 4	64±16	44±16	39±18	60±16
1.08	18± 4	72±16	46±16	41±17	55±16
1.20	20± 4	71±22	49±13	41±17	49±19
1.32	19± 4	74±20	54±11	42±18	51±18
1.44	20± 3	76±13	61±14	49±22	56±15
1.56	22± 3	71±16	67±15	50±21	57±18
1.68	25± 6	76±15	70±11	51±28	57±13
1.80	27± 7	74±13	74±12	54±25	63± 7
1.92	32± 9	76± 9	78± 9	58±25	64± 8
2.04	39±10	76± 9	85± 5	60±26	64± 6
2.16	47±17	80±12	87± 3	54±24	67± 8
2.28	47±12	81±12	89± 2	58±20	67± 8
2.40	44± 8	82±14	90± 0	59±22	69± 7
2.52	42± 6	82±14	90± 0	60±21	70± 7
2.64	42± 6	82±14	89± 1	60±21	73± 7
2.76	42± 4	84± 9	90± 0	59±22	72± 6
2.88	40± 5	86± 7	89± 2	62±19	73± 8
3.00	41± 8	87± 5	89± 2	68±20	75± 6
3.12	42± 6	88± 4	90± 0	70±18	73± 3
3.24	44± 5	89± 2	90± 0	72±18	77± 5
3.36	60± 6	90± 0	90± 0	72±19	83± 8
3.48	66± 8	90± 0	90± 0	74±18	81± 7
3.60	70± 7	90± 0	90± 0	72±19	82± 7
3.72	67±10	90± 0	90± 0	72±19	88± 3

Legend same as in Table II.

it is in localization with the technique employed in this investigation.

Localization with the technique employed in this study is as consistent as localization with the telephone receiver technique.

General summary. Since differences between localization toward the left and toward the right can not be accounted for on the basis of differences in the stimuli; and since there are no significant differences in localization with the sources interchanged; and since localization with the technique used in this study is as consistent as that with the telephone receiver technique, it is safe to conclude that the technique employed in this investigation is valid.

Summary and Conclusions

Previous investigators who have studied binaural sound localization with a single impulse technique have agreed that sound localization is conditioned by some variation in the temporal factor; but they have assigned widely discrepant values to the critical points in the temporal series. They employed the telephone receiver technique which, because of resonance in the receivers, and in the auditory canals when the receivers are placed in contact with the ears, introduced conditions under which phase-differences may have influenced localization.

With a technique in which the possible conditions of simple phase relations were avoided by employing very short, high-pitched sounds as stimuli, binaural sound localization has been carefully analyzed. The following results have been obtained:

(1) When the stimuli are presented simultaneously or with an interval ranging up to approximately .06 σ, a single fused sound is localized in the median plane. According to *Klemm* (**14**, p. 125) the fused sound leaves the median plane when the temporal interval is only 2 $\sigma\sigma$. *Von Hornbostel* and *Wertheimer* (**11**, p. 389), *Hecht* (**10**, p. 110), and *Wittmann* (**30**, p. 67) state that localization remains medial until the interval is 30 $\sigma\sigma$. But Bennett reports that "rightness" and "leftness" are not perceived with any degree of certainty until the interval ranges

above .1 σ. These differences are probably due to the fact that the various investigators worked under different conditions and with different types of apparatus and technique.

(2) When the temporal disjunction is greater than .06 σ, called the threshold of localization, trained *O*s generally localize two distinct sounds. One of these sounds is stronger in intensity and lower in pitch than that which corresponds to the stimuli. The other is very faint and higher in pitch than that which corresponds to the stimuli. As the temporal disjunction is gradually increased above the localization threshold, the stronger sound describes approximately a 90° arc from the median plane to the aural axis, on the side of the ear that is first stimulated, while the weaker one makes a similar course on the other side of the median plane. These dichotic sounds come to the aural axis when the temporal interval is approximately 2 σ. Previous investigators have reported only the stronger sound. *Von Hornbostel* and *Wertheimer* (**11**, p. 389) report that the fused sound comes to the aural axis when the interval is .63 σ. *Wittmann* (**30**, p. 68) states that the fused sound does not come to the aural axis until the interval is 2.6 σ.

(3) The relationship between the angular displacements of the two binaural sounds and the temporal differences is approximately linear, as the temporal interval ranges between .06 σ and 1.2 σ. According to *Wittmann* (**30**, p. 57) and *von Hornbostel* and *Wertheimer* (**11**, p. 389), the relationship between the angular displacement of the fused sound and temporal differences is approximately linear.

(4) Before the weaker phantom sound was discovered, by chance, in the course of the present investigation, the *O*s reported a single, the stronger, phantom sound which divided into dichotic sounds, when the interval was 2.36 ± .98 σ. *Klemm* (**14**, p. 126) states that the fused sound divides into dichotic sounds when the interval is 2 σ. *Banister* (**2**, p. 150) reports that the fused sound divides when the interval is approximately 1.75 σ. According to *Wittmann* (**30**, p. 68) the sound does not divide until the interval is 16.5 σ.

(5) As the temporal interval is gradually increased from .06 σ, the stronger binaural sound gradually becomes weaker and higher in pitch while the weaker sound gradually becomes stronger in intensity and lower in pitch until they are equally intense and qualitatively alike, or diotic, when the interval is 9.48 ± 3.5 σ. According to *Wittmann* (30, p. 32) the fused sound gradually becomes weaker in intensity and higher in pitch as it moves from the median plane to the aural axis. *Klemm* (14, p. 128) reports that the fused sound becomes lower in pitch and weaker in intensity as it moves away from the median plane. *Banister* (2, p. 147) reports that the dichotic sounds finally become equally intense.

(6) When the temporal interval ranges above 9.48 ± .98 σ, the diotic threshold, double sounds, illusory movements of sound, and a third phantom sound are perceived.

Theoretical Interpretations

The results of this analysis substantiate further the time-theory of sound localization; *i.e.,* they indicate that temporal differences are important conditions for the perception of direction in auditory space. Just how the two ears function together to reduce the physical variant of time to localization in space is a question that has been variously answered.

Boring (5) claims that because of inhibitory processes the cortical excitation is greater for the prior stimulus, and that a temporal difference is therefore reduced to an intensive difference at the auditory center; *i.e.,* the prior stimulus to a greater or less degree according to the temporal interval, inhibits the later. The physiology of such inhibition, Boring admits, is not clear. He rests his claim on a psychological theory of inhibition which defines attention as predetermined, so that when stimuli are too numerous for the range of attention, only that stimulus is effective for which the organism is in some way predetermined; and which holds that every mental process and presumably its neural correlate undergoes a process of culmination and decay.

Klemm (14) states that a temporal difference may be reducable

to an intensive difference because of interference in the later stimulated ear, due either to bone conduction or to diffraction of sound about the head. He bases this statement upon the fact that in a simultaneous presentation of the stimuli the fused sound is louder than it is in a disjunctive presentation; and that when the disjunction is great enough to introduce two sounds, the sound on the side of the ear first stimulated is the louder. While Klemm leans toward this interpretation he admits that there are insurmountable difficulties; *viz.*, (1) the problem of bone conduction, and (2) the question of the effectiveness of very slight intensive differences. It will be remembered in this connection that in the foregoing experiment, which dealt with sound shadows in localization, a shadow larger than that cast by the head did not affect the localization of the fused sound.

Klemm also states that a temporal difference may in reality be a phase-difference because telephone receiver membranes such as he used may not be "deadbeat." Or phase-differences may result from continued vibratory movements of the fluids in the inner ear; and still another possibility that a temporal difference may be a phase-difference lies in the perseveration of the neural currents over the auditory tracts. Klemm discounts this interpretation on the basis that the phenomena of localization should change periodically with increasing temporal difference and with changes in the pitch of the stimulating sounds. The reverse interpretation has more often been made, *i.e.*, a phase-difference has been interpreted as a difference in the time of arrival of like phases at the ears, by *Stewart* (25), *Banister* (3), *Bowlker* (6), and *Halverson* (8). To reduce a temporal difference to a phase-difference might involve a further reduction of phase-difference to intensive difference, as *Seashore* (21) and *Halverson* (9) have suggested, or the acceptance of *Stewart's* (24) theory that wave-phase is directly perceived.

Halverson (8) believes that it is possible to reduce the physical variants of phase, time, and intensity to some common factor of auditory effectiveness. In the writer's opinion, this position is well taken, since it has been experimentally demonstrated many

times that each of the different physical variants are effective, when the other variants are controlled, in determining localization. Each of the physical variants has its optimal effectiveness. According to *Stewart* (25) phase-difference is the effective agent in the localization of pure tones ranging from 100 to 1,200 d.v., above which he says intensity-difference may or may not be the effective agent. *Wilson* and *Meyers* (29) and *Raleigh* (18) have also defined limits for the effectiveness of phase-difference in determining localization. The limits of the effectiveness of differences in intensity have not been defined; but when the intensive difference is relatively large, double lateral localization results. The limits of the effectiveness of temporal difference, according to Klemm, are 2 $\sigma\sigma$, for the lower limit, and 2 σ for the upper limit. Von Hornbostel and Wertheimer place these limits at 30 $\sigma\sigma$ and .63 σ, respectively. Wittmann places these limits at 30 $\sigma\sigma$ and 2.6 σ. The results of the present investigation show that temporal differences are effective when the interval ranges between .06 σ and 2.36 σ. Since all of these variants are effective in determining localization, it seems reasonable to assume that the two ears serve as mediating agencies for transmitting to the central nervous system effects corresponding to any one or more of these slight differences in the stimuli. This would then result in localization and would make the problems of binaural sound localization parallel to the problems of binocular perception of distance.[6] The factor, according to this hypothesis, that is common to time, phase, and intensity as agents in localization is one of slight physical difference in the stimuli.

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[6] *Cf.* Seashore (22, p. 69) and Watt (26, p. 177).

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A NOTE IN REGARD TO THE EXTENT OF THE VIBRATO [1]

by

HAROLD M. WILLIAMS

Schoen (1) was the first to publish measurements in connection with the nature and form of the vibrato in artistic singing. The fact that the results of succeeding investigators have differed consistently from his findings in regard to the extent of the pitch oscillation in the vibrato has made it necessary to repeat his measurements and discover, if possible, the source of error in his work. According to Schoen " in terms of vibrations the extent of the vibrato is about the same throughout the entire range of the voice, being 10, 11, and 12 d.v. for the lowest, middle, and highest ranges respectively, or 0.25, 0.15, 0.09 in terms of a part of a whole tone " (1, p. 251). *Metfessel* (2), on the other hand, finds that this extent " varies from one-quarter to over one half tone." He also states that " the results of the present research show that the extent in terms of part of a tone is far more constant throughout the range of the voice than in terms of vibrations " (2, p. 25).

Schoen's conclusions are based on tonoscopic readings of the pitch variations of several well-known singers in their renditions of various tones from the Bach-Gounod "Ave Maria." The curves given on page 248 of his paper are generic or theoretical curves drawn from a knowledge of the total extent of pitch and the number of oscillations per second of the vibrato in the following tones from the composition: b'', d'', f♯', (the 87th, 13th and 28th tones). The singers are, Eames, Alda, Gluck, Destinn and Melba. The vertical lines in his graphs represent the average

[1] This study was undertaken at the suggestion of Dean Carl E. Seashore.

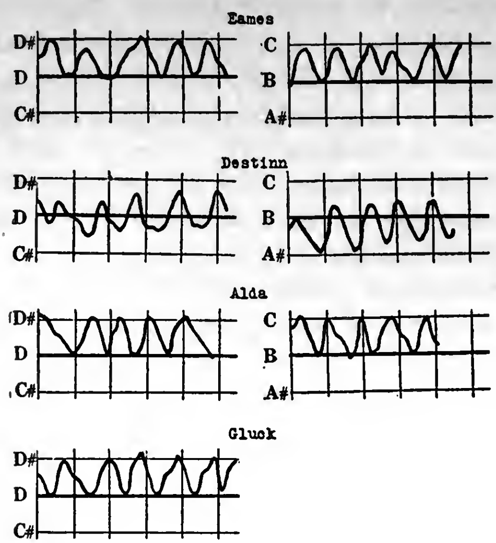
extent of pitch oscillation for several tones in each of the three ranges, high, middle, and low.

Seven of the fifteen tones measured by Schoen were selected by the present writer as being a fairly representative sampling, at least for showing the general tendency. The following tones were the ones selected: d′′ as sung by Eames, Alda, Destinn and Gluck; b′′ as sung by Eames, Destinn and Alda. The tones selected were all photographed by the phonophotographic technique developed by Metfessel (2), *Simon* (3) and *Kwalwasser* (4) in the Iowa Laboratory, and the pitch variations plotted graphically according to their methods. Each sampling was restricted to one revolution of the camera drum which corresponds in phonograph photography to one revolution of the phonograph record. At an original recording rate of 78 r.p.m. for the phonograph record the samplings were therefore .77 seconds in length. This was enough to include in nearly all cases at least five complete vibrato oscillations. The samplings were taken from approximately the middle of each tone.

The reliability of the phonelescope technique has been adequately determined by Metfessel and Simon. Metfessel has shown that the errors from mechanical factors are negligible, while Simon has furnished data with regard to the reliability of reading wave-lengths. In the method commonly used the wave-lengths are read with a glass millimeter scale, tenths of a millimeter being estimated by the reader. As Simon has shown, a degree of accuracy sufficient for all ordinary purposes can be obtained by grouping two, three, or four successive waves. In the present study d′′ was read in groups of two, giving a reliability of 1/26 of a tone, b′′ in groups of four, giving a reliability of 1/28 of a tone, in terms of A.D.

The graphs of the " raw " readings are always quite rough and uneven, though showing very clearly the large fluctuation known as the vibrato. The graphs were further smoothed by the use of the mathematical technique known as the method of the " moving average" as described by Kelley (5, p. 27 f.f.). This is the best simple technique available for determining the most

probable general tendency of a curve, and for smoothing out minor periodicities. The graphs below represent the result of the readings of the tones photographed and twice smoothed, once by grouping and once by the method of the moving average.



Graphic representation of the samplings showing extent of the vibrato. The distance between lines on the graphs is one-half tone. The heavy line represents the theoretical pitch of the tones sung.

The distance between lines on the graph is one-half tone. The mean extent of pitch oscillation and the average deviation for each tone under consideration are summarized in the table below, in terms of vibrations and in terms of fractional parts of a tone.

A comparison between the mean pitch of each tone as ascertained by Schoen and by this study shows an excellent agreement. There is left, then, to be accounted for only the discrepancy between the findings of Schoen and these of later investigators in regard to the extent of the pitch oscillation in the vibrato, in terms of d.v. In his measurements Schoen used the tonoscope (6). The phonograph was run at six revolutions per minute and the pitch variations were observed directly on the tonoscope, the values of the means and the two extremes being

called out to an assistant. The following paragraphs represent an attempt to demonstrate a source of error which Schoen may have overlooked in his investigation.

The tonoscope has 110 rows of holes, the first row having 110 holes, the second row 111 holes, *etc.* It reads to an accuracy of 1 d.v. in the first octave, (without estimating values less than one d.v.) of 2 d.v. in the second octave, and so forth. In the second octave, however, the holes seem to be only half as far apart as in the first octave, each dot having moved only half the distance to the position of the next dot when the second flash of light from the intermittent light occurs. This gives a clue as to the proper octave.

TABLE I. *Extent of the vibrato in terms of vibration, and in terms of % of a whole tone*

Singer	Tone "D" vibrations	Tone "D" % of a tone	Tone "B" vibrations	Tone "B" % of a tone
Eames	24.8±2.2 A.D.	59	12 ±1.6	40
Destinn	21.2±1.6	50	18.8±1.3	62
Alda	23.0±1.8	54	15.7±1.2	52
Gluck	29.0±2.2	65		

In only one case is the extent less than one-half tone.

In the case of frequencies below the lower limit of the instrument, on the other hand, a different situation holds. For example, assuming, for the sake of simplicity, 100 d.v. to be the lower limit on the tonoscope, each flash of the light at 100 d.v., will reveal a dot in the exact position occupied by the preceding dot in the flash before. A frequency of 50 d.v. will, however, reveal the same picture, except that each dot has moved down two places. For a tone of 33 1/3 d.v. each dot will have moved down three places. In every case, however, the pattern remains the same in appearance to the eye. Only by the ear can the pitch be determined.

The tones under consideration, when the phonograph is run at the very slow rate specified by Schoen, 6 r.p.m., will have frequencies of the order of 25 to 75 d.v. They will, therefore, fall within the range which has the peculiarity mentioned above. It is obvious that, in order to get the true value of the vibrato

in terms of parts of a tone, deviations from the mean pitch at these levels must be multiplied by certain constants. A single illustration carried through will serve to make the point clear.

At the speed of 6 r.p.m., b'' as sung by Eames will have a mean pitch of 77.7 d.v., d'' a mean pitch of 45.5 d.v., and f♯ a mean pitch of 28.1 d.v. Assuming that in each case the true vibrato oscillation is exactly a tone, at the level of 77.7 one half tone equals an interval of 4.4 d.v. (tempered intonaation), at the level of 45.5 a half tone equals 2.6 d.v., and at the level of 28.1, it equals 1.5 d.v. Our assumption of a vibrato of a tone will give us a pitch oscillation, therefore, in the three cases of 77.7 ± 4.4, 45.5 ± 2.6, and 28.1 ± 1.5 d.v. The three tones will range as follows: 73.3 to 82.1, 42.9 to 47.1, 26.6 to 29.6. But, since 73.3 is below the range of the tonoscope, it will register on the scale on the same row as its next multiple, which is 146.6. In the same way 82.1 will register as 164.2. This gives an apparent range of 17.6. In the same way, when we multiply by three, the apparent range of the vibrato for the tone 45.5 will be from 128.7 to 141.3, a range of 12.6 d.v. When multiplied by 4, the apparent range of the vibrato for tone 28.1 is from 106.4 to 118.4. In other words, the vibrato will, under these circumstances, appear to have approximately the same extent in terms of vibrations in all three regions, unless the proper calculations are made to compensate for the abovementioned peculiarity of the tonoscope. An hypothetical extent for the vibrato of 50 per cent. of a tone, which is not far from the true average extent, gives us values which approximate quite closely those found by Schoen, namely, 10, 11, and 12 d.v.

This appears to be the only possible explanation for the discrepancy between the results of Schoen's study and those of all later investigators. It serves also to explain the error made in inferring that the extent of the vibrato is constant in terms of vibrations rather than in terms of parts of a tone. As a correction to Schoen's original article, then, the value assigned by him for the extent of the vibrato at the lowest range, 25 per cent. of a tone, should be multiplied by 2, the value given for

the middle ranges, by 3, and the value given for the highest range, 9 per cent. of a tone, by 4. For an explanation similar to this see Metfessel (2, p. 15). Metfessel explained the discrepancy in terms of overtones, whereas, in the opinion of the present writer, overtones are not essential to the account.

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